PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

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PROFESSOR CHRISTISON, Vice-President, in the Chair.

The following Address, at the request of the Council, was delivered by the Rev. H. B. TRISTRAM on the Recent Scientific Researches in Palestine.

Notwithstanding the intense interest of the topography of Palestine, and above all else of the Holy City itself, yet it is only within the life-time of the present generation that any attempt has been made, systematically and scientifically, to explore its sacred and historical sites.

Centuries after the foundations of ancient Rome had been traced, years after the temples of Attica had been searched and rifled by Lord Elgin of their fairest ornaments, we knew no more of the topography of Jerusalem than could be gathered from the monkish chroniclers, or from Reland and Maundrell, and dreamt not that a buried city lay beneath the streets of modern Zion, as little revealed by the contour of the buildings on the surface as were the palaces beneath the sand mounds of Assyria. It was not until 1838 that any attempt was made to explore any part of Palestine in a strictly scientific manner, whether with reference to its topography, archæology, or natural phenomena. In that year the American Dr Robinson, in company with the missionary Eli Smith, made his first Biblical researches, which he continued in 1852. Whatever may be our opinion of the value of Dr Robinson's archæological theories, he remains still the prince of Palestine geographers in all that relates to general topography; and in many cases where his conclusions have subsequently been rejected as unsound, his researches have been the means of identifying or correcting the positions of long lost sites. He was followed in 1848 by his countryman, Commodore Lynch, whose celebrated expedition down the Jordan and upon the Dead Sea is familiar to every student. In 1851–52, the Dutch officer, C. W. M. Van de Velde, made his survey, and provided for us the first accurate map of modern Palestine. His work has been the basis upon which most subsequent explorers have proceeded. In the following year, Mr Porter explored the wonderful country of the Hauran, or Bashan, and revealed to us many of its ruined cities. In these regions east of Jordan Mr Cyril Graham has made yet more important discoveries, which he has published chiefly in the Transactions of the Geographical Society.

Of those who have not personally visited the country, one writer, Carl Ritter, has rendered invaluable service by his exhaustive work on the Comparative Geography of Palestine, which has recently been rendered accessible to English students by Mr Gage's translation, published by Messrs Clark, and which is almost indispensable to every student of the subject.

The Rev. G. Williams, in his learned work "The Holy City," is almost the only writer who has hitherto devoted both learning, archæological knowledge, and a long residence in the Holy City, to the elucidation of the topography of ancient Jerusalem itself, all other writers having been deficient in one or other of these indispensable requisites for investigating the vexed question of the sacred sites.

In 1864 the Palestine Exploration Fund was commenced, for the purpose of the complete and thorough exploration of the Holy Land, both as regards its archæology, topography, manners and customs, geology, and natural history. It is perfectly impossible for ordinary travellers from private resources, still less for tourists, to accomplish these great objects, especially as regards archæology.

Before proceeding to other branches of research, I will endeavour shortly to explain what the Palestine Exploration Fund has already accomplished in the examination of Jerusalem itself.

In 1864–65, Captain Wilson, R.E., completed the ordnance survey of Jerusalem, at the cost of Miss Burdett Coutts.

In December 1865, Captain Wilson again returned, and was occupied until the following May in the general survey of the country.

Shortly after his return, Lieutenant Warren, R.E., was sent out, and has been devoting himself with unwearied diligence in the exploration of ancient Jerusalem, as well as to the survey of Gilead, Moab, and Philistia.

We may classify his discoveries in Jerusalem itself under two heads—1*st*, The traces of ancient walls and streets; and 2*d*, The ancient water supply and hidden streams of the city.

The main key to the topography of ancient Jerusalem is the course of the Tyropœon valley, frequently mentioned by Josephus, and described by him as bisecting the city, and dividing the upper market, or Zion, from the lower market, or Akra. Its lower end has been identified by all explorers as coming out between Mount Zion and Moriah. But the whole portion of it within the walls, which was once a deep depression dividing the upper city from the lower and from the Temple, is now but a slight depression, and in places not at all to be detected on the surface. More than any other existing city, Jerusalem has been whelmed under heaps of rubbish. Its repeated sieges and destructions, and its rebuilding with fresh material from the inexhaustible quarries on every side, have completely hidden all remains of the city of Herod, still more of that of Ezra and of Solomon.

Walls and gates lie low beneath the accumulated debris of centuries. Towers, fortifications, houses, have come down in the course of ages, and the old ruin has always been taken as the foundation of the new edifice. Captain Wilson and Lieutenant Warren have sunk more than thirty shafts in different directions, and everywhere, after penetrating through many feet of rubbish and ruin, they have found old foundations, cisterns, or watercourses.

At the south-west corner of the Temple area, a shaft has been sunk 85 feet, and then a gallery run along, which shows the bottom of the valley to be actually 115 feet beneath the present surface, at the point known as Robinson's arch.

Josephus tells us of a magnificent viaduct across the Tyropœon, leading from the Temple area to Mount Zion. Dr Robinson was the first to detect the remains of this viaduct, in three stones, evidently the first spring of an arch, near the south-west corner of the Temple area.

This seemed to place the site of the Temple at this angle of the platform, until Captain Wilson discovered, much farther to the north, near to the Jews' wailing-place, another arch entire, but buried under the rubbish on which the modern houses stand.

Continuing Captain Wilson's researches, and sinking 50 feet below the present surface, Mr Warren has traced this magnificent viaduct by the remains of its arches right across the chasm, thus setting this question for ever at rest.

Robinson's arch appears to have been a single arch, probably for the purpose of supporting a gallery alongside of the Temple wall.

The Tyropœon, moreover, from the excavations in its upper part, proves to have been very different in form from anything hitherto supposed, viz., tolerably flat for the greater part of its width, with ample space for a lower city, as described by Josephus, and suddenly descending close below the Temple wall to a narrow gallery of great depth. Here projected Robinson's arch, the pier of which seems to have been reached, and two courses of huge dressed stone found *in situ*. What must have been the effect to one standing on the pinnacle of the Temple, which here rose to a height of 150 feet, from a platform of solid masonry 150 feet lower down, so that he looked down from an elevation of 300 feet sheer into the valley beneath? "See what manner of stones and what buildings were here."

Then, again, by the repeated sinking of shafts all over the surface of the sloping pier, which runs down from the Temple platform to the fount of the Virgin, that lower hill once known as Ophel, and an important suburb of the city, it is established by actual demonstration that the south wall of the sacred enclosure which contained the Temple is buried for more than half its depth beneath an accumulation of rubbish, probably the ruins of the successive buildings which once crowned it, and that if bared to its foundation, the wall would present an unbroken front of solid masonry of nearly 1000 feet long and 150 feet in height. The wall as it stands, with less than half that height emerging from the ground, has always been regarded as a marvel. What must it have been when entirely exposed to view! No wonder that prophets and psalmists should have rejoiced in the "walls" and "bulwarks" of the Temple, and that Tacitus should have described it as *modo arcis constructum.* "Walk about Zion, and go round about her; tell the towers thereof. Mark ye well her bulwarks, consider her palaces, that ye may tell it to the generations following."

Now, as the lower part of this wall is manifestly a substratum, in order to increase the platform space for the Temple area, which must always have been at its present elevation, that of the Sakhra, or central rock, the question naturally arises whether there are not subterranean chambers beneath those already known and described underneath the Aksa. Now, beneath the Huldah gate, in the south wall of the Haram, is an ancient double passage or tunnel. At the north end of this is a vaulted chamber 17 feet square, recently discovered, but further than this no explorations have yet been made.

To proceed to the south. The wall of Ophel has been discovered abutting on that of the Temple area at the south-east, and has been traced 300 feet from the Haram area to the south. A tower has been discovered, a face of 29 feet 6 inches projecting about 8 feet beyond the wall, built by Jotham, king of Judah (2 Chron. xxvii. 3). The bases on which the piers of the arches of the substructure rest on this point are 80 feet above the rock, and it is likely there is another system of older arches underneath those visible at present. The courses of this south wall are from 3 feet 3 inches to 4 feet in height; and 20 feet below the surface of the Haram walls, or 60 feet below the area of the Haram, has been found a passage from 12 to 16 feet high, and in it the check where a metal gate was swung, and the marks of its abrasion on the stones.

In the double passage is what is called the "Well of the Leaf," at the bottom of which was found a curious arch of tiles, perhaps an outlet to some subterranean flow of water.

Lieutenant Warren is inclined to doubt that the masonry west of Robinson's arch was a pier, as the ancient masonry is not in keeping with the Haram wall. It is 15 feet above the rock, and the remains of a house rest on the top of the wall. The pier itself is 12 feet 2 inches thick, and the distance to the Haram wall 41 feet 6 inches.

Higher up, under Wilson's arch, the depth of the shaft till the rock is reached is 53 feet. At 50 feet 9 inches the base course of the Haram wall is let into the rock, and the span of the arches is within 6 inches the same as that of Robinson's arch.

In the centre of the city the accumulation of debris is scarcely less. In Muristan the shaft was sunk 70 feet before the rock was reached; and some years since the foundation for the English Church on Mount Zion was sunk for to a depth of 40 feet, when the arch of an old aqueduct was struck.

When, however, we get to the north at the Damascus gate, we find the foundation of the old wall outside it 10½ feet thick, only 3 feet beneath the present roadway, which, it must be noted, is sunk many feet beneath the surrounding heaps. At the foot of this wall, which is very unlike the Temple walls in masonry, a Templar's cross was found, and it is probably the Crusaders' wall, destroyed when they were compelled to leave the city.

We next come to the water supply.

The most distant as well as the most important source of the supply are Solomon's pools at Etham, near Bethlehem, now called El Burak. These three artificial tanks are well known, being respectively 380, 423, and 582 feet in length, 236 feet wide, and from 28 to 50 feet deep. They are supplied from a subterranean fountain, "the sealed fountain," three-quarters of a mile to the north-west; from which a subterranean passage, leading to the pools of Solomon, has been explored by Lieutenant Warren. In the course of his researches he has met with traces which lead to the belief that there is another sealed fountain yet to be discovered to the south-east, which contributes to the supply of the pools, at the head of which is a spring or outflow, carefully concealed in the rock, from which the water rushes to the upper pool. There are two lines of aqueduct at different levels, the higher one now in ruins, from these cisterns to Bethlehem and Jerusalem. The lower level aqueduct, in which water still flows, follows the windings of the hill sides by Bethlehem to the valley of Hinnom, which it crosses upon nine low arches above the lower pool of Gihon. From hence it sweeps round the southern brow of Zion, and enters the city on the side above the Tyropœon, where it can be traced for a short distance, partly hewn in the rock, and partly supported on masonry against the side of the cliff. It enters the Haram at the gates of the Chain. Just outside of this gate is a large subterranean cistern, 84 by 42 feet, and 24 feet deep, doubtless supplied in olden times by this aqueduct. Again, near the Fountain gate, is a tank of enormous dimensions, which is within a few feet of the aqueduct, which could easily fill it. (*Burg al Kibreyt.*)

But this was not the only supply of water on which Jerusalem had to depend. In all the history of its sieges, there is not any allusion to the inhabitants suffering from thirst. All confirms the truth of Strabo's words—*ἐντὸς μὲν εὔϋδρον, ἐκτὸς δὲ παντελῶς διψηρόν.*

There is first the upper pool of Gihon, spoken of by Isaiah, who went forth to meet Ahaz at the end of the conduit of the *upper pool* in the highway of the fuller's field. We also read that Hezekiah stopped the upper outflow of the waters of Gihon, and brought it down to the west side of the city of David. Here, too, Rabshakeh stood when he delivered the insolent message of his master the king of Assyria.

The lower pool of Gihon is also mentioned by Isaiah (xxii. 9): "Ye gathered together the waters of the lower pool."

There is next the vast system of tanks by the *fosse* of Antonia, north of the temple area, the pool of Bethesda, connected with the enormous system of tanks under the Haram area, so far as we can conjecture. Then there is the pool of Hezekiah, close to Muristan, which probably supplied Akra, or the lower city. This was probably fed by the outflow from the aqueduct from the pool of Gihon after supplying the city of David. It is mentioned in 2 Chron. xxxii. 30, that Hezekiah stopped the upper outflow of the waters of Gihon, and brought it straight down to the west side of the city of David. And in expectation of the siege by Sennacherib, "he took counsel with his princes and his mighty men to stop the waters of the fountain which were without the city. . . . So there was gathered much people together who stopped all the fountains and the brook that ran through the midst of the land, saying, Why should the king of Assyria come and find much water?"

We have not yet discovered these hidden sources, but Lieutenant Warren has explored at Lifta (Nephtoah?), two miles north-west of Jerusalem, a perpendicular chasm 155 feet deep, through which water constantly drains and runs out of sight at once, which cleft he thinks partially artificial, and connected with a similar cleft he examined near the Russian buildings north of the pool of Gihon and close to it.

Underneath the temple area is a vast system of caverns, one of which, "the sea," deserves the name of a beautiful subterranean lake, while the whole hill is pierced by wells, and honeycombed by rock-hewn cisterns.

To the west of the temple is the Bath of Healing (Hummām esh Shefa), where there is a series of vaulted cisterns, with passages and deep wells, from which the water rises.

Below Ophel is the Fountain of the Virgin, celebrated for its irregular and uncertain flow, and which appears to be fed from some outflow underneath Mount Moriah. Hence is a subterranean channel leading to the pool of Siloam, which has been explored, and another strange tunnel discovered by Lieutenant Warren, connecting it with a vast tank.

Siloah, mentioned in Scripture as by the king's gardens, has again a subterranean connection with En Rogel or Bir Eyub, Joab's Fountain, discovered by Lieutenant Warren, while this too appears to have had a yet further extension by a second hidden channel to the south. 500 yards below Bir Eyub, Lieutenant Warren opened a spring, and at a depth of 12 feet a stone suddenly rolled away, and revealed a staircase, about 25 feet deep, and at the bottom, passages leading north and south. Steps and passages are cut in the rock; the latter are about 6 feet high, and may have been for the purpose of leading off the Jerusalem waters out of the reach of an enemy invading the city. The passage north has been explored for 107 feet, and is full of silt without stones.

Then again, under the Well of the Leaf, beneath the Haram, are ducts discovered, plainly leading to some yet undiscovered cisterns.

Again, at the upper end of the Tyropœon valley, Lieutenant Warren has discovered, at the bottom of a shaft of 50 feet, a drain with a stream of sweet water running through it with a constant flow, not from the baths, nor yet from the aqueduct of Solomon's Pools, but which must be from a spring at the head of the valley within the city.

At the south-east of the *cœnaculum* he has discovered, 50 feet above the level of the present aqueduct, what is no doubt the remains of Solomon's original aqueduct from the pools of Etam. This he has penetrated 250 feet in one direction, and 200 in the other. Often the explorer had to wriggle through lying on his back, at others to crawl on hands and knees. In parts it is built of masonry, in parts cut out of solid rock, and generally of a semi-cylindrical shape; but in many parts it has the peculiar shoulders only before noticed under the triple gateway, and recently discovered also in the channel leading to Tekoah from Etam, almost certainly Solomonic in date. In part of the passage he could stand upright, it being 10 or 12 feet high, with the remains of two sets of stones for covering, as shown in Mr Piazzi Smyth's work on the Great Pyramid; the stones at the sides being of great size, 12 feet by 6. This channel cannot be so late as the Roman period; it is evidently of the most ancient construction. It is built in little spaces, as if the work had been commenced at two or three points, and had not been directed properly. The plaster is still in good preservation. This channel must have been of great consequence, both from the distance it is driven under ground, and from the well-cut shafts which lead to it. The main supply of Jerusalem was probably obtained by it, as it is perfectly concealed from any besieger.

In the Muristan a system of tanks has been discovered 53 feet below the surface. Four have already been examined, from 50 to 68 feet long and 17 feet wide, with a flight of twenty-five steps down the side, with water in some, hard mud in others, and the plaster as fresh and impervious to water as if just constructed.

Most interesting have been the explorations of the Virgin's Fountain under Ophel. Lieutenant Warren discovered in the channel from this fountain a passage branching from that to Siloam of the most extraordinary character. For 50 feet the tunnel runs. Then for 17 feet more is the new passage, opening upon a shaft 6 feet by 4, and 42 feet high, to the landing in a wide cave some 30 feet high in its slope. After creeping along for 40 feet, a passage was discovered 8 feet wide, and from 3 to 4 feet high, cut in form of a depressed arch out of rocks. After following this for 40 feet more, a wall of rough masonry was found across it. From this wall the passage rises at an angle of 45°, still cut as before out of rock, with toe holes cut in it. Fifty feet up there is another wall of rough masonry, opening into a vaulted chamber, with a descent of 20 feet from the opening, and at the bottom a smaller one 8 feet deeper, with a passage blocked up, which has not yet been explored. In the horizontal part of the passage were found curious antique glass lamps, water jars, a little pile of charcoal as if for cooking, and a cooking-dish glazed inside. Evidently this had been used as a refuge. Overhanging the shaft was an iron ring, to which a rope might have been attached for hauling up water.

Lieutenant Warren considers that which has usually been held to be the aqueduct from the Temple to the Virgin's Fount, to have been rather an aqueduct for the supply of Ophel, as evidenced by a shaft and draw-well.

While exploring the tunnel between the Virgin's Fount and Siloam, the same gurgling sound of water was heard which has been noticed by others; but the hidden channel, which has thus been detected by sound at intervals from the Damascus gate to Siloam, yet remains to be revealed.

Now, fragmentary as all these discoveries are, they indicate an amount of ancient remains below the surface, which cannot but excite our hopes, and stimulate us to strain every nerve to lay bare such interesting relics. We may find but little yet to throw direct light on the lives we so cherish, and any detail of which we so dearly prize. True, but it must be recollected that the exploration in Jerusalem is at present in the condition of a puzzle or joining map, of which only half a dozen pieces are found out of sixty or seventy. Find the others, and the whole can be put together, and will then be intelligible enough. Extend to the other parts of the city the researches here begun, and the sites of the Temple, of the Tombs of the Kings of Judah, of the Holy Sepulchre, of the Pool of Bethesda, will be problems no longer.

As Mr Grove remarks "as I read Lieutenant Warren's accounts, I seem to feel the ancient city within my grasp, to know for a certainty that its very houses, and streets, and water-courses, all the ancient life of its structures, its hills and its ravines, are lying buried, like some enchanted person, beneath that singular and solemn tomb. The cliff (says Dean Stanley) which Joab climbed, the streets which David trod, and along which Athaliah was hurried, the catacombs of the kings of Judah, the very Via Dolorosa itself, of which not even the shadow of a likeness can be found in the upper air of the modern city—all these are doubtless there. Wherever we probe, go deep enough, and we come on some solid substance of curious and ancient kind. A stone suddenly rolls away and reveals staircases, passages, subterranean halls in the heart of the rock, leading to who knows what repositories of treasure. An almost invisible crevice in the hill turns out to be an enchanted cave, 150 feet deep, containing perhaps the hidden fountain of the water supply of Jerusalem."

But outside Jerusalem how much remains for us to learn! In the valley of the Kedron, Lieutenant Warren has sunk for 50 feet through debris again and again, without meeting the native rock. He is inclined to believe the tradition of the Mishna respecting the bridge of the red heifer, and is now searching for the foundations of the arches or pillars, as it will probably prove to have been a pier bridge, if it really existed.

Of archæological researches in other parts of the country there is little to be noticed. The few examinations which have hitherto been made here and there all encourage us to hope for a rich harvest wherever the work shall be systematically undertaken. Thus Captain Wilson's exposure of the ancient synagogues of Capernaum has, with a very trifling amount of excavation, revealed to us the extent and magnificence of an edifice associated in our minds with events scarcely less deeply interesting than those which cluster round the temple of Jerusalem. The remains of Seileon (Shiloh), Samaria, Gerizim, the cities of the Philistines, Beisan, (Bethshean), Heshbon, Rabbath-Ammon, all by their extent and interest invite careful exploration.

In physical research, much too has recently been accomplished. It is only within the last four years that we have become aware of the wonderful variety of the physical phenomena of Palestine, of the very interesting geological record contained in the region surrounding the Jordan valley, and especially of the unique fauna and flora confined within the narrow limits of the cliffs which overhang the Dead Sea. There, in a depression 1300 feet below the level of the Mediterranean, we find an almost tropical fauna and flora, containing a few Indian, many Abyssinian, and several peculiar and unique species in all the different forms of life, which make, in fact, a *tropical outlier*, just as the tops of the mountains, 10,000 feet high, supply us in their botany with boreal outliers. Not the least interesting fact ascertained is the peculiar character of the fishes of the Sea of Galilee and the Jordan, many of which are peculiar, some Nilotic; but whether peculiar or African, exhibiting remarkable affinities with the African forms of the entire and central lakes as far south as the Nyanza and the Zambesi river; and pointing to a time, perhaps at the close of the Miocene period, when this depression formed the northernmost of a long chain of fresh-water lakes, extending as far as south-east Africa.

The following Gentlemen were elected Fellows of the Society:—

J. W. Laidlaw, Esq.
W. Williams, Esq.

Monday, 17th February 1868.

Professor PLAYFAIR, C.B., Vice-President, in the Chair.

The Chairman said—Gentlemen, before beginning the business of this meeting, I wish to refer to the lamented death of two of the office-bearers of this Society. Dr Burt, a member of our Council, was well known as a public-spirited citizen, who had long usefully devoted himself to the development of our institutions, and who, by his genial disposition and honesty of character, endeared himself to all those who enjoyed his acquaintance. The other loss has had a more marked relation to us, because in the death of Sir David Brewster this Society has lost its President, and this country one of her most distinguished philosophers. This is not the time to refer to the benefits which Sir David Brewster has bestowed upon science. These have been so numerous and important that we may expect a special evening to be devoted to this consideration. If Professor Tait, who is so capable to do justice to the merits of his deceased friend, were to undertake this subject, I am sure the Royal Society would hail with pleasure the announcement of his intention. Such a record of the achievements of a great philosopher has a much higher purpose than that of an éloge, for while they become landmarks in the progress of science over new and untrodden paths, they indicate the methods by which further progress is to be attained. Sir David Brewster entered this Society so long ago as 1808, and has been a constant contributor tc its Transactions. It is worthy of notice that in the year 1808 the three candidates who became Fellows of the Society were, James Wardrop, surgeon, now in London, David Brewster, and Humphry Davy. In announcing to us at the opening of the session the death of Faraday, Sir David said that there was only one person living who had, like Faraday, taken all the medals of the Royal Society of London—the Copley, Rumford, and Royal medals. There is no one living now to claim this high honour, for the "one" so modestly hinted at was himself. In Brewster and Faraday the nation has suffered a heavy loss. Both were great philosophers and ardent Christians. We point to them as conclusive proofs that science and infidelity are not akin. I dare not trust myself to speak of the last days of Brewster. The perfect calmness and kindly consideration with which he wrote farewell letters to the public bodies which had honoured themselves by honouring him during life, were perhaps to have been looked for in one who viewed death as a means of attaining a higher and purer knowledge of God and of His works. But it is given to few men to possess their mental faculties unclouded to the last. A week before his death I had a long letter in his own handwriting, showing the liveliest interest in the affairs of the University, and in some optical discoveries regarding which he frequently corresponded with me. A few days after, while his mind was still clear, but his bodily frame weaker, he dictated a letter to the Council of this Society, in which he took a touching leave of his old associates and of the Society itself, and left to it, as a precious legacy, a research, nearly completed, which formed the death-bed study of the old philosopher. I am sure that the Society would not have wished to commence the business of this evening without some allusion to the death of their venerable President, and without some expression of sympathy with his widow and family. I therefore invite, from the body of the Society, a resolution which will record the sense of our own sorrow, and of our strong sympathy with that deeper personal affliction which is felt by the widow and children of so great and good a man.

Professor Sir James Y. Simpson moved the following resolution, which was seconded by Thomas Stevenson, Esq., and unanimously adopted:—

"The Royal Society of Edinburgh hereby record their deep sense of the great loss which the Society have sustained by the death of their late venerable and esteemed President, Sir David Brewster.

"Early in life an earnest worker and a happy discoverer in some of the most recondite fields of physical knowledge, Sir David Brewster has, during the last sixty and more years, continued with ceaseless energy to pour into the contemporary stream of science and literature a series of contributions of rare excellence and originality. At last he has passed from among us, as ripe in fame as in years; for he has reaped all the highest academic and other distinctions, both domestic and foreign, which a British philosopher can possibly win, and in his chosen departments of research he has left behind him no name more illustrious than his own.

"The Society further resolve to send a copy of this minute to Lady Brewster and the other members of Sir David Brewster's family, at the same time expressing their sincere sympathy with them in their late bereavement."

The following Communications were read:—

1. Observations relative to the Desireableness of Transporting from Alexandria to Britain the prostrate Obelisk presented to George IV. by Mahomed Ali Pacha. By Colonel Sir J. E. Alexander, K.C.L.S.

In the month of September last (1867), when visiting the Great Exhibition in Paris, I was particularly struck with the fine appearance of the obelisk of Luxor in the Place de la Concorde, and I thought that as the French had taken the trouble and gone to the expense of moving this highly interesting monolith, it was a reflection on our nation and on the engineering skill of Britain that the prostrate obelisk at Alexandria (one of Cleopatra's needles, as it is commonly termed), was not occupying a place of honour in England or Scotland.

This obelisk was presented to George IV. many years ago by Mahomed Ali Pacha, who also generously offered to move it on rollers to the sea, from which it is 200 or 300 yards distant, and embarking it on rafts and lighters, convey it to a vessel for transport to England.

The state of public affairs at the time, perhaps, prevented the accomplishment of this enterprise; now the time may be more favourable for it.

Sir Gardiner Wilkinson and other writers on Egypt and its antiquities are of opinion that Cleopatra's needles (one of which is upright) were brought by one of the Ptolemies from Heliopolis, near Cairo, to decorate a palace at Alexandria. On the obelisks appear the names of Thomes III. (B.C. 1463), of Remeses the Great, and of Oserei II. (B.C. 1232), long before Cleopatra's time. Sandys, who travelled in 1610, calls the prostrate obelisk "Pharaoh's needle," and says "it is half-buried in rubbish." It is of red granite, and, looking down a hole, its top is seen with crowned hawks sculptured on it. Lord Nugent, writing in 1845, says the hieroglyphics on three sides are well preserved. Colonel Ayton, of H.M. Bombay Engineers, informed me that whilst in Egypt in 1862, and whilst there was an idea of a memorial for Prince Albert first started, Mr Clark, of the telegraph department, uncovered the prostrate obelisk, removed the sand and rubbish from it, found the hieroglyphics on three sides in good preservation, and, as the obelisk was not then wanted, he covered it up again.

This obelisk, with others, is well ascertained to have been quarried at Syene, at the extreme boundary of Upper Egypt. It is not easy to find out how the hieroglyphics were graven on such a hard surface, and what was the process of hardening the bronze tools used for this purpose. The Messrs Macdonald of Aberdeen, and other workers in granite in this country, might be able to explain this: possibly the assistance of emery powder was brought in.

Denon alludes to Cleopatra's needles, and says they might be moved without difficulty, and form interesting trophies. To remove works of art from countries where they form ornaments and are conspicuous objects of interest is quite unworthy of a great people; but the obelisk in question lies in dishonour among low huts in the outskirts of Alexandria, and might well be spared to ornament one of our capital cities. In a conversation with Mr David Laing, the well-known antiquary, about it, he suggested the open space in front of the British Museum as the most appropriate for it. Still, it might not be sufficiently seen there—further west might be better, or in our Charlotte Square.

I corresponded with Mr Newton, the keeper of antiquities, British Museum, about the obelisk, and he writes—" It seems to me that if, by public subscription, a sufficient sum could be raised to transport this obelisk to England, it would be a just matter of national satisfaction; but you will understand that, while this may be a case fully justifying an appeal to the public for a subscription, it may not be one sufficiently strong to justify the trustees and officers of the British Museum in moving in the matter officially, because we have to make so many applications to the Treasury for grants for excavations, &c."

I communicated with the Peninsular and Oriental Steam Navigation Company regarding the means of transporting the obelisk, and the secretary for the managing directors states—" We would beg to suggest that the matter should be referred by you to the Foreign Office, whose agents have made all the necessary calculations on the subject, and without whose permission nothing could be done."

The Foreign Office was accordingly communicated with, and an answer was returned that the matter is now under the consideration of Lord Stanley.

The eminent civil engineer, Professor Macquorn Rankine, was asked what he thought of the means of transporting the obelisk, and he said—" I regret I cannot form any opinion whatsoever as to the best way of transporting the obelisk without having detailed information which, I believe, I could not obtain except by visiting the spot where it lies. The subject is undoubtedly one of very great interest, and I should very much like to be present when it is discussed."

In the Royal United Service Institution, London, I found thirteen large plans, carefully drawn, illustrating how, by means of inclined planes, a flat-bottomed vessel, machinery for raising the obelisk on a pedestal, &c., it could be sent to and set up in England. These plans are supposed to have been prepared in 1820 by Captain Boswell, R.N., for the Government, but no action was then taken in the matter.

It appears to me (having studied and been employed formerly as an engineer) that there might be no need for a vessel being built on purpose to carry the obelisk. A large Clyde lighter, raised upon, might transport it across the Bay of Biscay in summer; or if an old ship, sufficiently seaworthy, is got, and the masts taken out of her, and the beams cut across, the obelisk might be taken alongside, raised, and lowered into her, iron beams being ready, with bolts and screws, to connect and secure the cut beams of the vessel; then towed by a steamer to England. Once there, little difficulty would ensue before it occupied a place of distinction; but not necessarily on a pedestal, which would change its original character through giving it additional height. It is 64 feet long, weighs 284 tons, and is 7 to 8 feet square at the base of the shaft.

I understand that in an apartment in the Louvre part of the machinery is preserved by means of which the transport of the French obelisk was effected. This could be seen, or even lent to assist our engineers, and save heavy costs; and this need not be heavy, unless with gross mismanagement and a mere job made of it. Honestly gone about, the cost would be moderate.

Lord Stanley wrote me that he was not aware that the Parliament would vote a sum of money to move the obelisk. This might be asked, however.

I quite concur with Professor Piazzi Smyth in denouncing the barbarism of breaking off pieces of and carrying away Egyptian antiques; but I think we might remove the prostrate obelisk hidden and buried in the sand, leaving, of course, the twin obelisk set up in its place, and always most interesting as a "Cleopatra's needle." The prostrate one might be converted into building materials ere long, if not looked after.

Lately, in Glasgow, I made myself acquainted with the engineer of the Clyde Navigation, Mr A. Duncan. I went over the matter with him of the means of transporting the obelisk, and I suggested an iron casing or vessel built round it. He approved of this, and on my asking him to give his ideas on the matter—also to look at the plans from the United Service Institution—he kindly consented to do so; and his clear and excellent method for carrying out what is so much desired—the removal of the obelisk to Britain—is placed before the Royal Society of Edinburgh.*

2. On the Temperature of the Common Fowl (*Gallus domesticus*). By the late Dr John Davy. Communicated by Professor Allman.

Before proceeding to read Dr Davy's paper, Professor Allman made the following remarks:—

The duty has devolved on me of being the medium through which the paper just announced should be communicated to the Society—a duty not unmixed with melancholy; for when the distinguished author of that paper had placed it in the hands of the Council, there was nothing to prevent our looking forward to a continuance of those labours with whose results the pages of your Transactions have been so often enriched. But the pen which had worked unwearyingly for nearly sixty years is at last still; and the paper now before you closes in for ever the earthly labours of its author.

It has been suggested that, under these circumstances, it would be right to accompany the communication of Dr Davy's paper by a few words setting forth the salient points in his scientific life, and it is in accordance with this suggestion that I have drawn up the following short note. For the facts which it contain I am mainly indebted to his son-in-law, Professor Rolleston, of Oxford.

Dr John Davy was born at Penzance in May 1791; he died at his residence near Ambleside in January 1868. He was therefore in his seventy-seventh year at the time of his death.

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* Sir William Wylde, in a pamphlet sent me by Sir James Simpson, states that, in the year 1839, he proposed that the obelisk should form a Nelson testimonial, with sphinxes at its base; two of which, after the capture of Alexandria in 1801, were built into the wall of the Custom-House near the principal landing-place. These might be recovered also without much difficulty, as they are in a manner buried where they are. At St Petersburg I saw the granite rock on which the statue of Peter the Great stands, and got the details of the manner of its transport.

His scientific labours began in controversy, and we find him, in 1811, defending the views of his brother, Mr (afterwards Sir) Humphry Davy, regarding the nature of potassium and sodium, and the composition of hydrochloric, or, as it was then called, oxymuriatic acid. His advocacy of his brother's views was in reply to the objections urged against them by Mr Murray, at that time a lecturer on Chemistry in Edinburgh. His letters on the subject were published in Nicholson's Journal, and the controversy ran through several successive numbers of that periodical.

It was at this time that he was studying medicine in Edinburgh, where he became a member and one of the annual Presidents of the Royal Medical Society—a society composed of students, and which has numbered amongst its members some of the most distinguished names by which medical science has been advanced during the present century. Among Davy's contemporaries in the Medical Society were Richard Bright and Marshall Hall. In 1814 he graduated in Edinburgh as M.D., with a thesis, "De Sanguine." It must have been about the period to which we now refer that he assisted his brother in the Royal Institution in London, and discovered chloro-carbonic gas. He became a Fellow of the Royal Institution of London on 17th February 1814.

During the Waterloo campaign in 1815, Dr Davy was attached to a general hospital in Brussels, and in the following year he was appointed staff-surgeon in Ceylon, where he remained until 1820. While there he accumulated materials for an account of Ceylon, which still continues a standard work on that island. During subsequent years he did duty on various foreign stations—the Ionian Islands, Malta, Constantinople, and the West Indies. He also served on home stations; and his value as a public servant was subsequently recognised by Government in his appointment to the post of Inspector-General of Army Hospitals.

In 1842 he was elected a Fellow of the Royal Society, Edinburgh.

The opportunities afforded by a life so singularly active and varied were not allowed to escape him; for, with an ardent love of inquiry, he possessed powers of observation which have rarely been surpassed, and a versatility which enabled him to direct these powers into the most diverse channels.

In comparative anatomy his name will be always associated with our knowledge of the circulation of the blood in the Amphibia; for though Meckel had previously discovered that the auricle in the *Pipa* or Surinam toad was divided into two chambers by a membranous septum, yet the German anatomist regarded this as an exception; and in common with every other anatomist, from the time of Harvey downwards, considered that the batrachian heart had, as a rule, only a single auricle, by which it was distinguished from that of the true reptiles. Davy, however, by showing that the auricle is double in the frog and in the common toad, established this important structural feature as an essential character of the Amphibia.

Dr Davy's extensive pathological experience afforded him the materials for his work on "Diseases of the Army." His Life of his brother Sir Humphry proves his aptitude for critical biography; while his numerous papers on Natural History, Chemistry, and Physiology, show still further how free from speciality was his active and inquiring mind.

It is his physiological researches, however, and more especially those on animal heat, which stand forth with greatest prominence in the long list of his scientific labours. His observations on temperature, which extend to almost every important group of the animal kingdom, and embrace animals in almost every possible condition, are among the most valuable with which this department of inquiry has been enriched, and will always continue to be referred to by the historian of physiological progress. Among the very earliest of his publications is a paper presented to the Royal Society in 1814, on the difference of temperature between arterial and venous blood, and his last—that now before you—is still on his favourite subject.

Dr Davy's place in the history of scientific discovery has none of the brilliancy of his brother's, but it will always be known as that of the uncompromising advocate of truth—the patient, painstaking, conscientious, and successful observer. The discovery of facts—the elucidation of positive truth—was his mission; he seldom indulged in speculation, and did not possess that genius for generalisation which shed such a lustre over the career of the great chemist.

The later years of his life were spent in his retirement at Lesketh How, near Ambleside. When I saw him there last autumn he had lost none of his love of research, and dwelt with all the delight of a young man upon the natural features of his neighbourhood; time seemed as yet to have leant but lightly on him, and his looks were no index to his years; but he was hardly what I had known him some years before, and was certainly sadder than had been his wont.

His last illness commenced with what he at first considered as only a severe attack of influenza; but alarming symptoms supervened, and against these no treatment had any power. Dr Rolleston writes to me that "he retained his faculties to the last, and to the last the watchword was, with him as with the Roman Emperor, 'Laboremus.'"

Abstract of the Paper.

The observations were made on birds of different ages and sexes, and under various conditions, and the following results were approximatively obtained:—

1. The average temperature of the common fowl is 107°·81.

2. The temperature of the sexes before maturity is comparatively high, being 108°·5, whilst that of the two sexes at this stage varies very little.

3. The temperature of the male, on the whole, irrespective of any particular age, is higher than that of the female, being as 108°·39 to 107°·3.

4. The temperature of the fully mature male is 108°·77.

5. The temperature of the laying hen is 107°·4.

6. During incubation the temperature falls, and is as low as 107°.

7. During moulting it rises to 108°·44.

The author further observed that the young fowl, soon after becoming tolerably fledged, and capable of securing adequate food, attains a comparatively high temperature, and that the temperature of nestlings is also comparatively high, facts which are scarcely in accordance with the views of Dr William Edwards, who, in his work "On the Influence of Physical Agents on Life," maintains that "the power of producing heat in warm-blooded animals is at its minimum at birth, and increases markedly until adult age."

3. Note on an Inequality. By Prof. Tait.

The tendency of the air contained in a soap-bubble is to expand in bulk, while that of the film is to contract in surface. Hence it becomes a curious question to decide whether both of these tendencies are not satisfied when two or more bubbles unite into one.

The excess of pressure inside a bubble, of radius r, over the pressure of the external air, is

$$p' - p = \frac{4\,\mathrm{T}}{r},$$

where T is the contractile force of the film.

Hence
$$\frac{p'}{p} = 1 + \frac{4\,\mathrm{T}}{pr}.$$

Now if r' be the radius of the sphere which the contained air would occupy at the pressure p, we have

$$\frac{p'}{p} = \frac{r'^3}{r^3};$$

so that

$$r'^3 = r^3 + \frac{4\,\mathrm{T}}{p}\,r^2.$$

The quantity $\frac{4\,\mathrm{T}}{p}$ is exceedingly small, so that if we call it $3\,e$, we have very nearly

$$r' = r + e.$$

That is, the quantity by which the radius of a bubble must be increased, so that the pressure of the contained air may fall to that of the atmosphere, is independent of the radius of the bubble (unless it be very small).

Suppose two bubbles of radii r_1 and r_2 to coalesce, and let r_3 be the radius of the single bubble formed, we have (by expressing that the whole content is unchanged).

$$r_3^3 + 3\,er_3^2 = r_1^3 + r_2^3 + 3\,er_1^2 + 3\,er_2^2.$$

Hence, if $\delta\mathrm{V}$ express the increase of volume, $\delta\mathrm{S}$ that of surface we have

$$\delta\mathrm{V} + e\,\delta\mathrm{S} \doteq 0.$$

Hence it appears that the volume increases and the surface contracts, or *vice versâ*, for they cannot *both* remain unaltered. And the physical circumstances show at once that the former is the case.

The formula just written shows that the volume increases by as much as that of a bubble, whose surface is equal to the diminution of surface, would change when it is so increased that the air it contains is at the pressure of the atmosphere.

It is obvious that the same process of physical reasoning may be applied to any number of soap-bubbles when made to unite; so that we may thus, without any analysis, conclude that for any number of positive quantities x, y, z, &c., we have always

$$x^2 + y^2 + z^2 + \ldots\ldots > (x^3 + y^3 + z^3 + \ldots\ldots)^{\frac{2}{3}}.$$

For if R be the radius of the bubble formed by the union of others of radii x, y, z, &c., we must have

$$R^2 < x^2 + y^2 + z^2 + \ldots\ldots$$

$$\text{and}\quad R^3 > x^3 + y^3 + z^3 + \ldots\ldots$$

from which the above result is evident.

Professor Tait exhibited to the Society a very easy mode of demonstrating some of Stewart's results regarding equality of absorption and radiation. Letters drawn with ink on a slip of platinum foil appeared brighter than the ground when the foil was heated in a large blowpipe flame, but darker than the ground on the other side of the foil. Thus, though glowing more brightly, the iron spots are really at a lower temperature than the foil beside them. The spectra of the two portions were described and their lengths compared, with the view of strengthening the proof; and the experiment was applied to suggest an explanation of the singular observation by which Secchi was led to conclude that iron is transparent at a red heat.

Monday, 2d March 1868.

DAVID MILNE-HOME, Esq., Vice-President, in the Chair.

The following Communications were read:—

1. Observations on the Spores of Cryptogamic Plants, and on the Reproductive Process in some Algæ and Fungi. By Professor Balfour.

In this paper the author alluded to the spore as the ultimate germinating cell of cryptogamic plants. The true spore is the product of impregnation, but there are also bodies called spores which, so far as known, do not depend on the process of fertilisation. Occasionally the term spore includes both the spore and the spore-case or sporangium. The structure of the spore was described, and its resemblance to the pollen grain, both as regards its anatomy and germination, was shown. Ciliated moving spores were considered, and reference was specially made to the movements of the non-ciliated spores of Helminthora, which, when discharged, continue to change their form for several hours like the Amœba, becoming in turn oblong, pyriform, rhomboidal, &c.,—lengthening and contracting, swelling at one point and shrinking at another.

The germination of spores was considered. Some of them were shown to produce a prothallus, and were hence called *Thalloid spores;* while others had no prothallus, and were therefore *Athalloid.* In the case of the former, the prothallus was sometimes produced externally, as in ferns, and hence the spore was *Exothalloid;* in other cases the prothallus was internal, as in Lycopods, and the spore was called *Endothalloid.*

In all the great divisions of cryptogamic plants, bodies called Antheridia and Archegonia have been detected,—the former usually containing spermatozoids.

The conjugation in Tyndaridea, Zygnema, and Mougeotia was then considered, and the function of a compound spore in some species of Vaucheria and in Fungi, such as *Rhizopus nigricans*. In the last-mentioned plant the compound spore is called a Zygospore.

Special attention was drawn to the fertilisation of Florideæ, as demonstrated by Bornet and Thuret. The formation of antheridia with corpuscles, and of a peculiar hair-like body called Trichogynium was noticed. The cell at the base of this latter organ, after fertilisation, is transformed into the Cystocarp, which is sometimes supported on cells called the Trichophore. The formation of the Cystocarp containing spores was exhibited in large drawings of Nemalion, Batrachospermum, Helminthoro, Liagora, Callithamnion, Bonnemaisonia, and Polysiphonia. It was shown that, in the case of Dudresnaya, there is a long Trichogynium with spiral coils near its base, and that the cells below that organ elongate so as to form flexuose filaments, which pass in a serpentine manner among the ramifications of the sea-weed, and come in contact with the fructiferous filaments which they are destined to fecundate. These conducting tubes are attached successively to the terminal cells of the filaments, becoming incorporated with them. Thus, the fructiferous filaments are united together by a sort of network of delicate tubes. These tubes convey the fertilising influence from the Trichogynium to the Ampullæ, from which the Cystocarps are formed. It was thus shown that the fecundating apparatus of the Florideæ consists of antheridia containing corpuscles, and of a small cellular body terminated by a unilocular hair or Trichogynium. Fecundation is produced by the union of antheridian corpuscles with the Trichogynium, followed by the development of the capsular fruit or Cystocarp.

In some cases, as in Nemalion, the fertilisation is direct, the influence of the antheridian corpuscle being at once conveyed by the Trichogynium to the rudimentary cell of the Cystocarp. In other cases, as in Dudresnaya, the action is less direct,—the influence of the antheridian corpuscles being conveyed by connecting tubes, which pass laterally from the base of the Trichogynium to numerous fructiferous filaments, on which the Cystocarps are finally developed.

The author then considered the case of the spores contained in the Thecæ of Mosses and Ferns. These spores have not been shown to be produced by direct fertilisation. In these plants there is a prothallus bearing the reproductive organs. The spermatozoids of the antheridian cells come into contact with the archegonial cell, and after fertilisation the cell remains attached to the prothallus, and from it is developed the stalk bearing the sporiferous theca in Mosses, and the sporangiferous frond in Ferns. These so-called spores seem to resemble buds. Remarks were made on the buds of the Marchantia, the gonidia of Lichens, and the tetraspores of Algæ, as well as on the production of separable buds in the higher classes of plants, and the production of ova and buds in Medusæ. The tendency, also, of the spores of anomalous ferns to produce fronds having the peculiar character of these varieties, was considered as indicating their alliance with buds.

The paper was illustrated by a large series of coloured drawings and by models, as well as recent specimens of ferns in various states, from the prothalloid condition up to the frondiparous state, bearing sporangia, and also at times producing viviparous buds.

2. Note on a Recent High Tide on the East Coast of Britain. By George Robertson, M. Inst. C.E. (Plate.)

The greatest height to which the tide has ever been known to rise at any given place is of the utmost consequence to the marine engineer. It forms an important element in the design and in the estimate for any works in the sea; and any error on this point might lead to the most lamentable results both to life and property, especially in the construction of embankments for the reclamation of land.

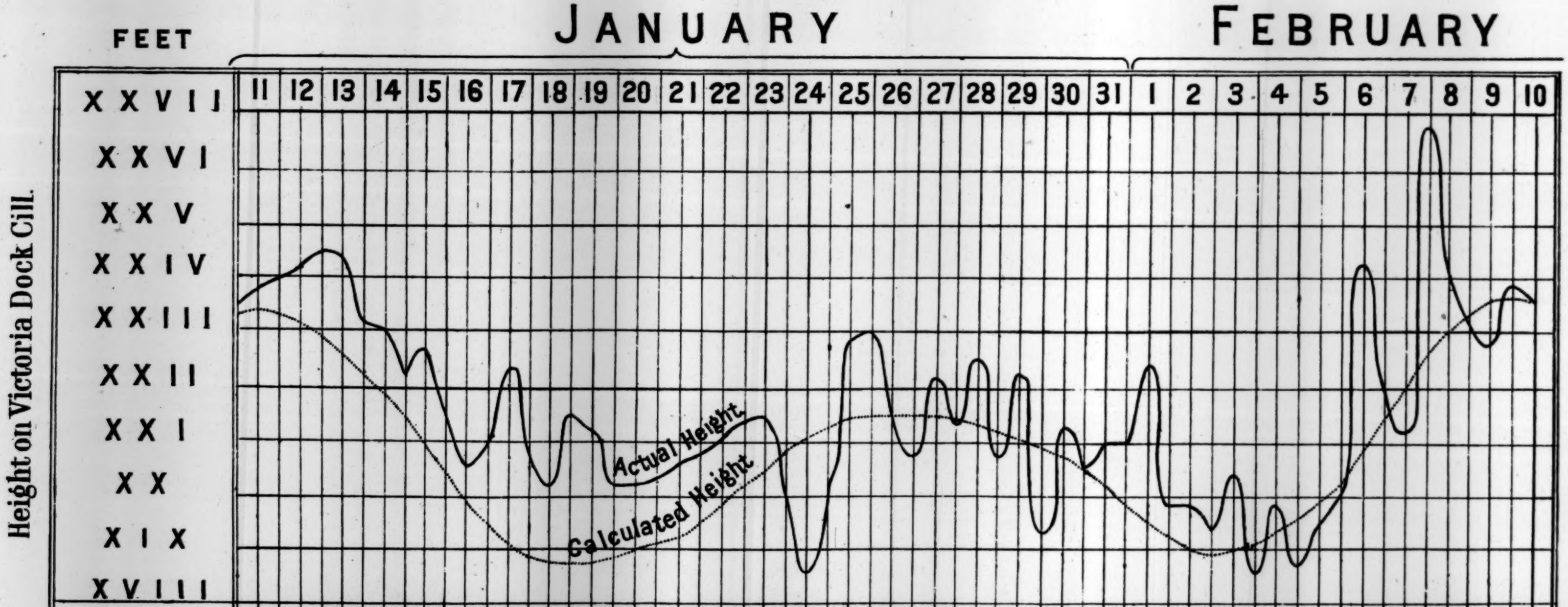
I have therefore thought that a notice of the tide of the 8th of last month might not be uninteresting to the Society, as it is one of the highest, if not indeed the highest, that has ever been recorded with accuracy on the east coast of Britain, and the most remarkable example with which I am acquainted of the effect of wind in raising the level of high water on that coast.

The months of January and February of this year have been remarkable for the long continuance and violence of westerly winds—at times approaching to the fury of a hurricane.

A long continuance of these winds has always the effect of

HIGH-WATER AT LEITH.

1868.



JANUARY	11		12		13		14		15	
Cal$^{d.}$ Height	23'3"	23'4"	23'3"	23'1"	22'10"	22'7"	22'3"	21'11"	21'5"	20'11"
Actual Height	23'5"	23'9"	23'11"	24'2"	24'5"	24'4"	23'1"	23'0"	22'3"	22'8"
Wind	ESE	ESE	WSW	SW	EbyS	NNW	SSW	SSW	SW	WSW

JANUARY	16		17		18		19		20	
Cal$^{d.}$ Height	20'5"	19'11"	19'6"	19'1"	18'10"	18'8"	18'8"	18'8"	18'9"	19'0"
Actual Height	21'7"	20'6"	20'11"	22'3"	20'8"	20'2"	21'5"	21'2"	20'3"	20'3"
Wind	SW	SW	SW	SW	SW	ESE	W	SW	NE	NNW

JANUARY	21		22		23		24		25	
Cal$^{d.}$ Height		19'3"	19'6"	19'10"	20'3"	20'6"	20'10"	21'1"	21'3"	21'5"
Actual Height		20'7"	20'9"	21'1"	21'4"	21'5"	20'3"	18'7"	20'2"	22'10"
Wind	WSW	WSW	NNE	NbyE	ESE	WSW	SSE	S	SW	WSW

JANUARY	26		27		28		29		30		31	
Cal$^{d.}$ Height	21'6"	21'6"	21'6"	21'5"	21'4"	21'3"	21'2"	21'1"	20'11"	20'8"	20'5"	20'1"
Actual Height	23'0"	21'6"	20'10"	22'2"	21'4"	22'6"	20'9"	22'3"	19'3"	21'9"	20'6"	21'0"
Wind	WSW	WSW	SW	WSW	WSW	WSW	WSW	WbyN	WSW	SW	WSW	SW

FEBRUARY	1		2		3		4		5	
Cal$^{d.}$ Height	19'10"	19'6"	19'3"	19'0"	18'11"	19'0"	19'1"	19'4"	19'9"	
Actual Height	21'0"	22'5"	19'9"	19'9"	19'4"	20'4"	18'7"	19'10"	18'8"	
Wind	WSW	WSW	WSW	SW	WbyS	SW	WSW	SW	WSW	WSW

FEBRUARY	6		7		8		9		10	
Cal$^{d.}$ Height	20'2"	20'9"	21'4"	22'0"	22'6"	23'0"	23'4"	23'8"	23'7"	23'6"
Actual Height	20'0"	24'3"	22'3"	21'2"	26'10"	24'4"	23'3"	22'9"	23'10"	23'6"
Wind	WSW	WSW	SSW	SW	ENE	N	SE	SW	WSW	WSW

Mould & Tod, Lith^{rs} Edinburgh

keeping up the tides round the coast of Britain; and if the wind then goes suddenly round to the north of west, it brings in the tidal wave from the Atlantic in great strength round the north of Scotland, and consequently raises the level of high water on the east coast to an abnormal height, more especially as the wave works its way southward to the shallow water and obliquely shelving shores of England.

The effect of these long-continued westerly winds on the tides culminated, on the morning of the 8th of February, in a tidal wave of perhaps unprecedented height on the east coast—the same wave which brought high water to the west of England and Ireland on the afternoon and evening of the 7th.

I shall endeavour to trace the height of this wave from the time it passed the Pentland Firth till it met, off Lowestoft, the counteracting low water of the wave which travels rounds the south of England.

Nothing remarkable in the height of the tide was noticed at Wick, where the tides seem to be more locally affected by gales from the S.W. to S.E., which raise the level of high water some 12 or 15 inches.

As the wave swept along the coast of Banffshire it increased in height, till at Aberdeen the level of high water was 3 feet 6 inches above the calculated height. High water took place here at 1.16 A.M., and was 24 feet 4 inches on the dock cill. The average of spring tides for five years at Aberdeen is 22 feet on the cill, but the tide of the 8th was not expected to rise so high as this, and it is the highest recorded since 1862, before which the records appear uncertain.

At Leith the tidal wave rose at 1.58 A.M. to the height of 26 feet 10 inches on the Victoria Dock cill. The calculated height, according to Reid's tables, which are those used at the port, should have been 22 feet 6 inches, so that the actual height was 4 feet 4 inches more than the calculated height; the greatest discrepancy I have ever heard of at Leith. Between the previous high water on the afternoon of the 7th and that on the morning of the 8th, there was no less a difference than 5 feet 8 inches. The extraordinary amount of this difference will be better understood if we bear in mind that it represents the ordinary total rise between high water of low neap tides and high water of high spring tides, an amount which it usually takes seven days and fourteen tides to accomplish by successive increasements; but which, in this case, was done at one bound.

On going over the tide-books at Leith for as far back as time has permitted, I find that during the last eighteen years there have been ninety tides, or five per annum, which reached the level of 25 feet on the Victoria Dock cill. Of these only two tides have been so high as 26 feet, viz.—the morning tide of October 26, 1865, and the morning tide of January 29, 1850. The latter tide reached the height of 26 feet 8 inches, or 2 inches lower than the one of the 8th ult.

A very high rise of tide is recorded on several of the charts as having taken place on July 18, 1829; but, on looking at Reid's Commercial List for July 21, 1829, I find that on the day recorded on the charts the tide was a very ordinary one, only rising 2 inches higher than was expected, and being exceeded by those following. So that there is some mistake which makes this high tide very doubtful, to say the least.

I have prepared a diagram of the height of high water at Leith for a tidal month, which shows the extraordinary state of oscillation in which the tides have been during the late boisterous weather. It will be observed that from the 11th to the 24th January the tides were all above the calculated height. On the 24th—the day of the great gale in Scotland—the wind went violently round to the south, and immediately the tides fell to more than 2 feet below their proper height, rising again as the wind went back to the west. After a fortnight of most unusual oscillation, the tide rose on the 8th February, with the turn of the wind to the north, to the unprecedented height alluded to in the paper.

I regret that I cannot give a diagram of the low waters for the same period, which would have been very valuable and instructive; but at Leith, as at many other ports, no regular record is kept of the low waters.

The height of the tidal wave of the 8th, when it had reached the mouth of the Tyne, in Northumberland, was much the same as at Leith—a little less, however, as might be expected from the configuration of the coast. The height of high water in Shields harbour was 4 feet more than expected, and was 3 feet 2 inches above high water of spring tides (Rennie's datum). This is 10 inches higher than any tide previously recorded. The tide on the 6th was also unusually high, being 3 feet 2 inches more than calculated.

At Sunderland the tide was 3 feet 11 inches more than the calculated height.

At Hull it was high water at 5.45 on the morning of the 8th, and the tide rose to a height of 30 feet 5 inches above the Humber Dock cill, or no less than 5 feet 5 inches more than was calculated. The tide of the previous day had risen only to 23 feet 4½ inches above the cill; so that we have at Hull the extraordinary difference of 7 feet ½ inch between two successive tides—the greatest amount of aberration I have hitherto discovered in the east coast tides.

The average height of springs at Hull is 26 feet 8 inches on the dock cill, there being very seldom tides of 28 feet. A list of the high tides at this port has been sent to me, and I find that since 1788 there have been only twenty-three tides which have reached a height corresponding to 29 feet on the Humber Dock cill—an average of only one tide in three and a half years. Of these twenty-three tides only one reached the height of 30 feet, and that was on the 18th February 1816, during a gale from the N.W. The tide was then 4 inches lower than that of the 8th ult., which is the highest tide on record at Hull.

The tidal wave appears to have reached its maximum about Hull, having travelled from Aberdeen in 4h. 29m.—according to Imray's tide-tables—exactly one hour faster than usual, as might be expected from so strong a flood. This is at the rate of nearly eighty miles per hour—a great speed for a wave of the first order in 50 fathoms water, and equal to what it usually is in 70 fathoms.

About this point, or perhaps nearer Yarmouth, the tide began to feel the counteracting influence of the low water of the wave which sweeps along the south and west of England.

By the time high water reached Lowestoft the level had apparently fallen a little; for though the tide was 20 feet 9 inches, or 4 feet 3 inches above the average, still this is 2 feet 3 inches less than the tide of the 2d December 1867, which is the highest on record at Lowestoft. On the 25th of January last—the day after the great storm in Scotland—the low water at Lowestoft was 4 feet below the average; and the harbour-master writes that they have had extraordinary fluctuations during the last three months.

When the wave reached the London Docks it was still extremely high, being 3 feet 6 inches above Trinity datum; but this is not unprecedented; for on January 20, 1850, it was an inch higher; and on November 12, 1852, it was 2 inches higher. This latter is the highest tide on record at the London Docks, and the water rose to 29 feet 1 inch on the dock cill, or 3 feet 8 inches above Trinity datum.

In examining the height of that portion of the great tidal wave from the Atlantic which sweeps along the south and west of England, I find a marked difference from the east coast tide. Instead of being the highest wave on record, it was a tide, slightly in some places only above the calculated height, and in no respect very remarkable as to height of high water.

At Cork the water did not rise above the average of springs either on the 7th or 8th February; but there was a great ebb, which left banks exposed in the harbour that had not been visible for years.

The wave which reached Portsmouth Dockyard at 11 P.M. on the 7th was the same as calculated; but on the morning of the 8th the tide rose to 21 feet 4 inches on the cill of Dock No. 6, which is the zero, or 1 foot 11 inches above calculation. This is not very remarkable, for there was a tide at Portsmouth on November 1840 which rose 2 feet higher, and is the highest on record.

At Dover the wave on the 8th was unusually high, being 2 feet 4 inches above the calculated height. High water was 21 feet 9 inches above low water of ordinary springs; but this is not the highest tide known, for the engineer of the Admiralty works writes that he has on record one of 22 feet, or 3 inches more.

That branch of the tidal wave which flows up St George's Channel towards the Irish Sea had nothing extraordinary in its height.

At Bristol the tide of the 7th was 29 feet 8 inches, or only 9 inches more than calculated. On the 8th it was 31 feet 1 inch—the height expected; this, too, at a place subject to very high tides, the highest on record being that of January 29, 1846, which was 37 feet 4 inches at the Cumberland Basin.

At Dublin the height on the 7th was 13 feet, and on the 8th 12 feet 7 inches above zero; but the engineer of the Ballast Board writes that this is nothing remarkable, and that he has a record of a tide 2 feet 9 inches higher than the one on the 7th ult.

The tides at Holyhead were much as usual, and were exceeded by those of the 9th and 10th.

At Liverpool the tide of the 7th was 1 foot 5 inches above calculation; but on the 8th it was 4 inches below what it ought to have been.

At Belfast the tide of the 7th was 11 feet above datum; on the 8th, 11 feet 2 inches; the average of tides being 10 feet. On the 13th and 14th the tides were at least 2 feet higher.

I have brought this notice of the recent high tide on the east coast before the Royal Society, not only for the purpose of having on record one of the most remarkable tide waves occasioned by wind in this country of which we have any authentic account, but also for a practical purpose.

The knowledge that there is a tidal wave of more than ordinary height on its way to any of the great commercial ports of Britain would be of great value to merchants and sailors. There are always vessels lying in harbours and docks of too great a draught of water to allow them to sail during neap tides, or even during low springs. Few ports in this country have sufficient depth of water to prevent large vessels being "neaped," as it is called. These vessels might be able to sail several days, perhaps ten or more, sooner than they could otherwise do, or be docked, as the case might be, were they timely warned of the fact that a tidal wave of more than the usual height was on its way to them, bringing, in extreme cases, a high spring during a low neap.

Information regarding high or low tides might just as easily be sent to each great port, without any extra expense, and with far greater certainty of prediction, as the warnings for high winds are at present, and might be signalled with equal simplicity.

There is always plenty of time for it. The tidal wave which reaches the west coast of Ireland about four o'clock at springs divides into two parts. The northern branch takes eight hours to reach Aberdeen, nine hours to reach Leith, fourteen hours to reach Hull, and twenty hours to reach London. The southern branch takes seven hours to reach Portsmouth, and seven hours to reach Liverpool.

The character of the winds prevailing at the time would, with a little experience, give a good idea of whether the coming tide would be felt most on the east or the west coast, and also of its probable height at different places.

The knowledge of the approach of an extraordinarily high tide might also occasionally be of some use in the preservation of property from its effects.

In these days of steam I am sure that information about tides would be quite as generally appreciated as information about winds, by both merchants and sailors. I believe that the Royal Society of Edinburgh would do a great practical service to the shipping interests of this country if they could prevail on the Meteorological Department of the Board of Trade to arrange that notice should be sent to all the principal ports of Britain, not only of the expected winds, but also of the expected tides, whether unusually high or unusually low. The storms of wind may or may not arrive—for the wind "bloweth where it listeth;" but the tidal waves flow and ebb without fail, with the speed of the hurricane, but the silence of the grave.

The following Gentlemen were elected Fellows of the Society:—

J. **Samson Gamgee**, Esq., Surgeon to Queen's Hospital, Birmingham.
Rev. D. T. K. **Drummond**, B.A. Oxon.
Rev. **Joseph Taylor Goodsir**.
Major **John H. M. Shaw Stewart**, Royal Engineers.

Monday, 16*th March* 1868.

DR CHRISTISON, Vice-President, in the Chair.

The following Communications were read :—

1. The Mean Pressure of the Atmosphere over the Globe for the Months and for the Year. Part I.—January, July, and the Year. By Alexander Buchan, M.A., Secretary to the Scottish Meteorological Society.

The three charts which were exhibited, showing, by isobarometric lines, the mean atmospheric pressure over the globe, during January, July, and the year, were constructed from observations made at 358 places thus distributed over the earth,—167 in Europe; 51 in Asia; 22 in Africa and adjoining islands; 35 in South America, West Indian Islands, and Atlantic; 63 in North America; and 20 in Australasia and Antarctic Ocean. Of the European stations, 12 are in Scotland, 14 in England, 27 in Austria, 12 in Italy, 10 in France, 10 in the Netherlands, 9 in Norway, and 57 in the Russian empire, &c. The list might have been largely increased; thus a larger number might have been given from the 80 Scottish stations; but the 12 given were judged sufficient to represent the mean atmospheric pressure of this country.

In the British islands the means were uniformly taken for the ten years from 1857 to 1866, in order that they might be strictly comparable with each other; and the means of several European stations were calculated for the same year. In the United States of America the means were uniform for the six years from 1854 to 1859. In selecting the stations, respect was had to the obtaining of a good mean,—that is, observations for a sufficient number of years to show, as nearly as possible, the true mean. For example, at Bombay, from 1847 to 1860, the lowest mean for July was 29·598 inches in 1851, and the highest 29·673 in 1853. This regularity in the pressure of the same month, from year to year, is a feature common to all tropical countries; and hence, in such places, a few years were accepted as a good mean. On the other hand, since the mean pressure at Reykjavik, Iceland, during January 1867, was 29·913 inches; during February, 29·359 inches; and during March, 30·037 inches, it is evident that a good many years are required to represent the mean of the month at Reykjavik. Hence, a very subordinate position, if any at all, has been given to observations from such places, unless they embraced a considerable number of years. The following are a few of the places, with the number of years, for which the means are given:—Sitka, 15; Algiers, 10; Hobart Town, 25; St Louis, Mauritius, 13; Bogoslovsk, 26; Nigni-Tagilsk, 21; Barnaul, 19; Nertchinsk, 18; Pekin, 14; Calcutta, 11; Tiflis, 14; Baku, 17; Alagir, 15; Jakobshavn, 10; Reykjavik, 13; Hammerfest, 13; St Petersburg, 19; Archangel, 18; Zlaloust, 28; Lugan, 22; Christiania, Cracow, and Kursk, 27; Brussels, 33; Gand, 26: Geneva, 25; Ahun, 34; Verona, 73; Bologna, 45; Milan, 25; Turin, 74; most of the Austrian stations, 14 to 18, &c.

Tables were prepared ruled with columns for the insertion of the following information:—1. The place and country; 2. The source or authority from whence the observations were obtained; 3. The number of years of the means; 4. These years specified; 5. The hours of observation; 6. The latitude; 7. The longitude; 8. The height above the sea in English feet; 9 to 21. The simple arithmetic means for each month and for the year.

For reducing to sea-level a table was prepared from the formula and Table XVI. given in Guyot's Meteorological and Physical Tables, D, p. 89. This table, calculated for each 5° F. of the temperature of the air—from 40° to 90°—was used in all cases where the height did not exceed 800 feet. For higher situations, the reduction was made by means of Dippe's method, as detailed in Guyot's tables, D, p. 60. With the correction for height was included the correction for daily range.

For many means I have been indebted to the labours and writings of Buys Ballot, Carl Jelinek, Dove, Quetelet, James, and Kuppfer, and particularly to Secchi's admirable abstracts, which have appeared from time to time in the "Bulletino Metereologico."

The means, so reduced, were then entered on charts of the globe drawn on Mercator's Projection, from which isobarometric lines were drawn for every tenth of an English inch in the difference of the pressure. The lines of 30 inches and upwards were coloured red, and the lines representing pressures less than 30 inches were coloured blue. Thus, the portions of the earth's surface where the pressure was above or below the average (30 inches) could be seen at once.

Mean Atmospheric Pressure for July.—The lowest pressures occur over the land and the highest over the ocean; or, the lowest pressures occur in the north hemisphere and the highest in the south hemisphere. The greatest extent of high pressure extends quite round the globe from a little south of the equator to 40° lat. south. In the South Atlantic, in lat. 20°, this rises to 30·348 inches; in the North Atlantic there is a corresponding area of high pressure rising at lat. 35° to full 30·348 inches; and this region of high pressure extends over the south-west of Europe and the south-east of the United States. There is also a region of high pressure in the North Pacific, but it only amounts to about 30·1 inches.

The greatest extent of low pressure occurs in Asia, amounting in the central regions of the Continent only to 29·5 inches. Pressures are also low in the interior of North America, and round the north and the south poles.

Mean Atmospheric Pressure for January.—In January the highest pressures are over the land and the lowest over the ocean; or, the highest are in the north hemisphere and the lowest in the south hemisphere. Thus, taken in a general sense, the mean pressures of July and January are reversed.

The region of highest pressure occurs in the interior of Asia, where it amounts to 30·4 inches, being thus nearly one inch greater in winter than in summer. The area of high barometer (above 30 inches) is continued westward through Europe south of the North Sea and the Baltic; the north of Africa; the North Atlantic, between 15° and 45° lat.; North America, except the north and north-west; the West India Islands; and the North Pacific, as far west probably as 150° long. W. The effect of the Mediterranean and adjoining seas, which are at this season warmer than the land, in lowering the mean winter pressure, and thus breaking the continuity of the isobarometric 30·2 inches, and preventing its extension from the Pacific to the north of the Lena, in Siberia, is very striking. All the charts show similar disturbances of the lines of equal pressure over the same region. There are two other regions, though of comparatively small extent, where the mean pressure exceeds 30 inches,—one in South Atlantic, and the other between South America and Australia.

There occurs in the North Atlantic an extensive diminution of pressure, which deepens northwards till the greatest depression (29·5 inches) is reached in Iceland.* It is this low pressure over the North Atlantic, together with the high pressures in North America, in the Atlantic south of lat. 40°, and in Asia, which furnishes the key to the winter climates of North America and Europe. Another and equally remarkable depression occurs in the North Pacific, having its course of greatest depression (29·6 inches) in the ocean between Kamschatka and Sitka in the north-west of America. The pressure is also under the average in the south of Africa and in South America.

The equatorial depression stretches quite across the globe in an irregular belt, which attains its greatest breadth in Africa and its least in the Pacific. In crossing the Indian Ocean it does not lie parallel to the equator, but slants from Tamatave in Madagascar 18° lat. S. to the coast of Sumatra, in 5° lat. S. It is in this trough that nearly all the tropical storms of the Indian Ocean have their origin.

Mean Atmospheric Pressure for the Year.—There are two broad belts of high pressure passing completely round the globe,—the one north, the other south of the equator,—enclosing between them the low pressure of the tropics, through the centre of which runs a narrow belt of still lower pressure, towards which the trade winds blow. The southern belt of high pressure lies nearly parallel to the equator, and is generally of uniform breadth throughout. But the belt north of the equator has a very irregular outline and great differences in its breadth and in its inclination to the equator,—these irregularities being due to the unequal distribution of land and water in the northern hemisphere.

Considered in a broad sense, there are only two regions of low pressure, one round each pole, bounded by or contained within the belts of high pressure just referred to. The most remarkable of these is the region of low pressure round the south pole, which probably is subject to little variation throughout the year. The depression in the neighbourhood of the north pole is divided into distinct centres, at each of which a diminution of pressure still further below the average pressure prevails. These are the north part of the Atlantic and the north part of the Pacific oceans. There is also a smaller area of low pressure in Hindustan, caused altogether by the low summer pressure of that region during the south-west monsoon. The centre of Asia is also a little below the average, owing to the very low summer pressure.

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* The diminution of pressure in this region is even greater than this in February.

These differences in the pressure arise from the unequal distribution either of the temperature or of the moisture of the atmosphere. Of these, considered as disturbing agents, by far the most important is the moisture,—thus giving to this element a paramount claim on our regard in studying winds, storms, and other atmospheric changes.

The relations of the barometric pressure to the prevailing winds, the varying temperature, and the rainfall in different parts of the world, were pointed out. Isobarometric and isothermal charts,—(1.) Of Europe and Western Asia, from the 18th to the 26th of December 1866; and (2.) Of Europe, from the 19th July to the 1st of August 1867, were exhibited; and it was shown that the remarkable deviations from the mean pressure of these months which then prevailed were accompanied with equally remarkable deviations from the mean temperature of the same months.

2. On the History of the Sun's Distance Determinations; and on Scriptural and Scientific Probabilities. By William Petrie, Esq. Communicated by Professor C. Piazzi Smyth, March 16, 1868.

This paper was accompanied by two diagrams, one of them representing more particularly the features attending on space, and the probable errors of several modern determinations therein; and the other representing the chronological order of the events, or progress made by man from the earliest times, down to the present, in ascertaining that most important of all questions in astronomy and general physics, viz., the true mean distance of the earth from the sun.

At the dawn of human science, properly so called, or of efforts in schools and societies to attain to a knowledge of nature by observation and measure, and when the Greeks sowed the first seeds of those methods which have since developed into the giant tree of modern science—that is, in the ages of 600 B.C. or 500 B.C.—their greatest philosopher concluded the sun to be hardly more than 10 or 12 miles from the surface of the earth. But they increased that estimate, as their science advanced, to 14,000 miles in 50 years; and to 5 millions of miles in 200 years more. At that point, however, knowledge remained stationary for 1800 years, and continued perfectly satisfied with the truth of that result,—until Kepler showed, in 1620 A.D., that the distance was more nearly 26 millions of miles; and La Caille, in 1750 A.D., that it was nearly 78 millions of miles; and the transit of Venus, in 1769, that it was 95 millions of miles.

This last determination was, however, over the truth, and subsequent measures have been reducing the quantity again; but at the same time oscillating, sometimes on one side and sometimes on the other, of a mean quantity nearly 92 millions of miles in length.

Now this quantity—which modern science seems to be approaching only, but has not yet definitely arrived at, or settled for society, and is not likely to be able to do so until the next good transit of Venus, in 1882 A.D.—is the quantity marked off for the sun's distance in the Great Pyramid of Jeezeh, by the ancient architect or designer thereof, and in so early a period of the world as to have been 1600 years before the very embryotic commencement of Greek, or any other known human, science; or at a time when it was perfectly impossible for any man then living, to have had any means at all sufficient to solve the problem even approximately!

How, then, was it solved? and whence came that marvellously accurate physical knowledge, perfect, apparently, at its very outset, and in a building which has lived through all historic times of all nations and peoples, but never been known to possess those scientific details before, not even by the Egyptians themselves?

This great question much occupies Mr Petrie, after having detailed clearly a large quantity of numerical and other particulars connected with the establishment of all the positive and material facts of the case; and his solution leads him ultimately to some interesting correlations with certain portions of Scripture exegesis.

3. Physical Proof that the Geometric Mean of any Number of Quantities is less than the Arithmetic Mean. By Professor Tait.

If a number of equal masses of the same material be given, at different temperatures, and enclosed in an envelope impervious to heat, they will finally assume a common temperature; which is the arithmetic mean of the initial temperatures, if the material be one whose specific heat does not vary with temperature.

But they may be brought to a common temperature by means of reversible thermodynamic engines employed to obtain the utmost amount of work from the initial unequal distribution. This question was first investigated by Thomson (*Phil. Mag.* 1853, "On the Restoration of Energy from an unequally heated Space"), and the application of his method to the present problem shows that the final common temperature of the masses, when as much work as possible has been obtained from them, is the geometric mean of the initial temperatures; but this investigation introduces the condition that the temperatures must be measured from the absolute zero.

Obviously the whole energy restored is proportional to the excess of the arithmetic over the geometric mean.

Far more complex analytical theorems may easily be proved by means of the above process; for instance, if t_1, t_2, ..., c_1, c_2 . . . . be any positive quantities, we have

$$\frac{c_1 t_1 + c_2 t_2 + \ldots .}{c_1 + c_2 + \ldots ..} > (t_1^{c_1} t_2^{c_2} \ldots .)^{\frac{1}{c_1 + c_2 + \ldots}}$$

4. On the Dissipation of Energy. By Professor Tait.

The paper contains some curious applications of the principle of dissipation to the conduction of heat, the connection of heat and electricity, thermo-electric currents, the electric convection of heat, &c. But in this abstract we confine ourselves to one very simple case of the conduction of heat, as the hypothesis on which it is investigated is fundamentally assumed in all the other applications.

If an infinite plate be kept permanently heated in layers, each of equal temperature throughout—the temperature rising gradually from one side to the other—the hypothesis is made that the temperatures of any three contiguous layers (of equal thickness) so adjust themselves that the least possible energy can be restored from the system of three. From this it immediately follows that if x_1 be the thickness of the plate, t_0 and t_1 the (absolute) temperatures of its sides; and if the specific heat be the same for all temperatures between t_0 and t_1: the temperature t at a distance x from the side at t_0 will be

$$t = t_0 \epsilon^{\frac{x}{x_1} \log. \frac{t_1}{t_0}}.$$

But if k be the conductivity of the substance, at temperature t, we have for the flux of heat

$$f = k\frac{dt}{dx} \propto kt.$$

This must be the same throughout the plate, because there is equilibrium of temperature, and therefore

$$k \propto \frac{1}{t}.$$

The only published experiments, so far as I am aware, by which this result can be tested, are the very valuable series by Forbes (*Trans. Roy. Soc., Edin.* 1864), which are, unfortunately, confined to iron. They agree uncommonly well with the above theoretical result, as the following short table shows :—

t	k	kt
290° C.	0·0164	4·76
330°	0·0130	4·24
400°	0·0110	4·40
440°	0·0105	4·58
476°	0·0100	4·76
561°	0·0090	5·04

No account has, in this abstract, been taken of the alteration of specific heat with temperature, which is as yet only approximately known, but which is applied in the paper to account completely for the increase of kt with temperature. As to the increase of kt at the low temperature of 290° C., it may be remarked that the first two or three numbers in Forbes' table are (as he points out) probably much less accurate than those which follow them, on account of the temperature at which they were obtained, which was but little above that of the atmosphere.

The following Gentlemen were elected Fellows of the Society:—

John Stevenson, Esq.
Rev. J. F. Montgomery.

Monday, 6th April 1868.

Professor KELLAND, Vice-President, in the Chair.

The following Communications were read:—

1. On the Flow of Solids, with the practical application in Forgings, &c. By M. Tresca, Sub-director of the Conservatoire des Arts et Metiers, Paris. Communicated by T. C. Archer, Esq.

2. On the Figures of Equilibrium of Liquid Films. By Sir David Brewster.

This paper is a continuation of that read on 4th February 1867, and already published in the Transactions. Instead, however, of the liquid films being formed within systems of wires, hollow glass vessels are here employed. By using a single or double cone of glass open at both ends, and inserting within it small bubbles blown from a quill or tube with a small aperture, regular binary, ternary, or quaternary systems of films are produced. Thus, by first obtaining a plane film within the conical vessel, and then inserting upon it four small bubbles, a system is produced consisting of four hollow films, of curious curvature, united to one another respectively by vertical plane surfaces, and connected in their centres by a common plane film, which, by adopting certain precautions, may, with a little care, be made to assume either a horizontal or a vertical position. This system may aptly be compared to that produced within the wire cube, and previously described. The analogy of the two systems may be further carried out by inserting one or more small bubbles in the centre of the system, which will, according to the form of the cone employed, assume a form approaching to that of a cube, and therefore analogous to the system shown in figs. 4 and 5 of Plate xxxiv., or to that of a pyramid (perfect or truncated), and therefore corresponding to the systems in 10 and 11 of the same plate.

The paper contains also some experiments on the motions of films when brought in contact with surfaces of glass under certain conditions. Thus, if a film be formed on the mouth of a wine-glass by dipping it in the solution, and be then covered over by a watch-glass of more or less convexity, so long as no contact is established between the film and the watch-glass no change will take place. But let the wine-glass be inverted so as to bring the film into a vertical position (the watch-glass being meantime kept firmly in position by the thumb), the film will now attach itself to the watch-glass at its lowest margin, and will run up its inner surface, describing thereon a series of curious curves. The upper part of the film will at the same time retreat down the inner surface of the wine-glass, and form a hollow segment of a sphere.

The cause of the phenomenon is the existence of a drop of the solution which enters the wine-glass in the operation of dipping, and remains at its bottom, and which, when the glass is inverted, runs down as far as its margin, and there comes in contact with the margin of the film. A connection is thus produced between the film and the watch-glass, which originates at a single point, but quickly extends in the manner mentioned. A variety of further experiments may be made on the film thus produced, several of which are described in the paper.

The last part of the paper consists of an account of some miscellaneous experiments chiefly on the tenacity of films when brought in contact with one another, or with surfaces of fluid, and then drawn apart, and the forms of the curves which in such circumstances they assume.

The paper is illustrated by a number of coloured sketches, done for the author by a lady residing near him at Melrose, who also prepared those for his previous papers, from which Plates xxxiii., xxxiv., xxxv., and xxxvi. were lithographed, and for whose kindness and trouble the author desired that his obligation should be mentioned.

The following Gentlemen were elected Fellows of the Society:—

John Dick Peddie, Esq.
Colonel Seton Guthrie.
Samuel Raleigh, Esq.
Thos. Smith Maccall, M.D., F.R.C.P.E.

Monday, 20th April 1868.

Professor PLAYFAIR, C.B., Vice-President, in the Chair.

The following Communications were read:—

1. Notice of Works designed by Sir Charles Hartley, C.E. for the Improvement of the Danube. By David Stevenson, Civil Engineer.

Having received from Sir Charles Hartley an interesting record of the works executed for the improvement of the Danube, to be presented to one of the Public Libraries in Edinburgh, I think its fittest destination is the Royal Society; and, on behalf of Sir Charles Hartley, I have to present the volumes on the table for the Society's acceptance.

It may not be uninteresting, in presenting these books, to describe, in a very few sentences, the work that has been executed for the improvement of the Danube, and the results that have been obtained.

In 1856 the "European Commission of the Danube" was appointed under the Treaty of Paris, and consisted of seven delegates, representing England, Austria, France, Prussia, Russia, Sardinia, and Turkey, and its object was to improve the bar of the river, and open the navigation to the traffic of all nations. The Danube, after flowing over a course of 1700 miles, and draining 300,000 square miles of country, enters the Black Sea by three separate mouths—the northern called the Kilia, the central the Sulina, and the southern the St George's mouth. The first duty of the Commission, with the advice of Sir Charles Hartley, who was appointed their engineer, was to select one of the three mouths for improvement, which was by no means an easy task, as each of them presented advantages peculiar to itself, and after much consideration the Sulina or central channel was selected, and although considerable difference of opinion existed as to the propriety of the choice the result has shown that the course adopted was judicious.

What gives professional interest to works on the Danube is their object—the improvement of a bar harbour, in all cases a troublesome engineering problem, due probably, in some measure at least, to the varying circumstances which the engineer has to consider in dealing with the formation and improvement of bars. In this country, for example, we find bar harbours at the Tyne, the Wear, and other similar localities, caused by the storms on our coasts, which, by the action of the waves on the bottom, throw up sand and shingle, and but for the scour produced by other agents a continuous line of beach would soon be formed across the mouths of our tidal rivers and inlets. But this action of the sea is counteracted by the scouring flow of the tidal and fresh-water currents, which, in spite of the waves, tend to keep open a deep channel through the beach. In this way the antagonistic action of the waves of the sea on the one hand, and the currents of the estuary or river on the other, produce the well-known feature of a submerged beach or sand-bank extending across our inlets, and having a channel through them of more or less depth, according to circumstances, which channel is termed "the bar." The conditions under which such accumulations are formed are as I have elsewhere stated :—*

1*st*, A bottom composed of materials easily moved;

2*d*, Water of depth so limited as to admit of the waves acting on the bottom; and

3*d*, Such exposure as shall allow of waves being generated of sufficient size to operate on the submerged materials.

But in such places as the Firth of Forth, for example, before the first two of these necessary conditions occur, viz., sufficient shallow water and presence of sand, the sea, owing to the configuration of the coast, is so landlocked that waves of sufficient size to produce the third condition cannot be generated, and hence in the Forth there is the phenomenon of a barless river.

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* Encyclopædia Britannica, Art. *Inland Navigation*.

At the Danube, a large river, discharging in ordinary flood no less than twenty millions of cubic feet of water per minute, enters a tideless sea, and we have a totally different class of phenomena to deal with. The river brings down an amount of detritus which has been ascertained by Sir Charles Hartley to be equal to 27 cubic inches per cubic yard, and to be equal to no less, in cases of high flood, than 600,000 cubic yards of solid deposit in 24 hours. Like the Mississippi and the Nile, the Danube owes its extensive delta to the gradual accretion of this sedimentary deposit, and the bar at its mouth is due to the same action. It therefore differs entirely from the bars in this country, as is well exemplified in the fact that, whereas in our harbours the bars are always *deepest* when the sea is calm and the rivers are *in flood*, and therefore most efficient as scouring agents, at the Danube the bar is, on the contrary, invariably *shallowest* when the river is in flood, because it is then charged with a larger amount of deposit.

Another feature of difference in the treatment of such a case as the Danube is to be found in the circumstance that there is no reversal of the current due to tidal influence, and therefore it is unnecessary, in fixing the direction of the piers, or indeed in designing any of the works, to provide for the admission of tidal water to act as a scour on its return to the ocean, a provision which always demands special attention in designing tidal works on our coasts.

The works executed at the Sulina mouth consist of a north pier 4640 feet in length, and a south pier 3000 feet in length, both built of *pierres perdues* surmounted by a timber staging, with an entrance between of 600 feet, and the slightness of their structure indicates the modified character of the waves to which they are exposed.

The works, which are understood to have cost about £100,000, are highly creditable to the talent and energy of Sir Charles Hartley, and have now been completed for five years, and their effect has been most satisfactory, as proved by the fact, that previous to their construction the depth on the bar never exceeded 11 feet and frequently fell to 8 feet; whereas, according to the last accounts from Sir Charles Hartley, the depth for the last five years has never been less than 15 feet, and has often been as much as 17½ feet.

It is obvious, however, that as the Danube must continue to bring down an enormous mass of detritus, so, in course of time the works which have proved so successful must be extended—an event which has been fully anticipated by its projectors, and in this respect we find an interesting difference between such works as the Danube piers and the harbour works of this country, for here the object being to prevent the waves from acting on the bottom, the engineer extends his works out into a depth of water where there is little or no disturbance of the bottom, and if this is once secured he may calculate on the increased depth of water remaining permanent, whereas, at the Danube the piers must be projected to keep pace with the gradually increasing delta at the river's mouth.

2. Remarks on two Pyramid Papers in the last published Number of the "Proceedings" of the Royal Society of Edinburgh. By Prof. Piazzi Smyth, April 20, 1868.

On the 10th of March last I had the honour of receiving, in the usual course of publication, a copy of the 75th Number of our Society's *Proceedings.*

Within that Number's few pages, there are no less than two distinct essays, mainly, if not entirely, on "*the Great Pyramid,* "*and my views respecting it.*"

Both these essays, though by able and ingenious men, are yet unfortunately based to such an extent on needlessly imperfect data, that the superintending Secretary might have justifiably returned the MSS. to their respective eminent authors, with suggestions for improvement in the improvable particulars, before publishing them under the auspices, and at the expense of the Society.

This course, however, was not followed; and the words of friendly advice, which might before have been offered in private, require now to be stated in public; for not only have the printed copies been distributed amongst the members, but one copy having reached the "Medical Times" of London, was commented on thus definitively in its impression of March 28, 1868:—

"SIR J. Y. SIMPSON ON THE PYRAMIDS.

"It is characteristic of the brilliant and diversified gifts of the "great Northern Professor, that he should have taken in hand the "demolition of a curious notion about the Great Pyramid of Egypt, "which has been promulgated by Professor Piazzi Smyth, and has "found favour with many religious persons. That notion is, that "the Pyramid was constructed under Divine guidance, to show to "all time a correct standard of weight and measure, and that the "coffer contained in the central chamber is an inspired measure of "capacity, and the Base of the Pyramid an inspired measure of "length, having a definite relation to the earth's polar axis. Sir "James Simpson shows, *in a paper read before the Royal Society* "*of Edinburgh*, that the Pyramid has all the characters of the "huge sepulchral monuments scattered over the earth; that the "coffer in the King's Chamber was a sarcophagus; that it contained "a body till despoiled by the Caliph El Mamoon about a thousand "years ago; that it is irregular in form, incapable of being exactly "measured, and hence no standard of measure. In fact, any one "who desires a treat, in seeing how a thorough '*craze*' is melted "away before common sense, should read Professor Simpson's "paper. The kind of argument of Professor Piazzi Smyth, to "show that the Pyramid was built by Noah, is one which we "thought peculiar to the medical profession:—Noah was a preacher "of righteousness; a just system of weights and measures is "righteous: *ergo*, Noah built the Pyramid!"

M. WACKERBARTH'S PAPER.

The first of the two papers printed by the Society appears, indeed, only in the shape of an abstract of the original essay; was composed by a native of Sweden, and is, in truth, such a *naive* production, in the total want of knowledge which it displays of the progress of all Pyramid research during the last three or four years;

—that I have thought it enough to send the author a letter, nearly to the following effect:—

" Dear Sir,—I have read to-day your communication *on the* " *Great Pyramid of Jeezeh, and my Views concerning it*, in the 75th " Number of the Proceedings of the Royal Society of Edinburgh " (pp. 235–238).

" So much ability appears in the Memoir, that I regret you had " not something better to work upon than those few pages of mine, " written four years ago, especially as I have since then visited the " Great Pyramid, examined it steadily during four months, and " published the results last year in full.

" Your time would surely be more usefully spent in criticising " what I now maintain, and have myself measured at the Great " Pyramid, than in running down details which I have long since " discarded, because proved to be without sufficient foundation.

" Some important leading points, however, both in theory and " induction, which I still hold implicitly, and always have held, " with regard to the Great Pyramid, since I first published any- " thing upon it—you seem to have entirely failed to apprehend. " As, for instance, the very broad and striking fact, that none of " the standards deduced from the Great Pyramid's earth-com- " mensurabilities, are similar to those of the idolatrous Egypt of " history. Even the stars of reference in the sky, are absolutely " and totally different. The very essence, in fact, of the Great " Pyramid theory, as to its earth-founded weights and measures " is, that they are actively opposed to those of profane Egypt, and " could not have been invented by, nor were they ever known to, " any of her idolatrous sons.

" Therefore you do not bring up anything new, either to the " theory or myself, when you discover and publish that some *one* " in particular of the Great Pyramid standards was not in general " use in ancient Egypt. Nor again do you really injure my next " argument about the high antiquity of the British *inch*, by show- " ing, as I also had done, that our *yard* is of comparatively recent " origin.

" If your special desire is to upset the metrical theory of the " Great Pyramid, prove, if you can, that I am largely wrong in " those measures of length and angle throughout the building, on

" which the theory is mainly founded,—and you may succeed in " your design; especially, too, if you can explain all the *well-* " measured and already explained facts still more accurately, on " some other single, consistent, and connected theory.

" But if, in place of doing any of these things, you merely pro- " duce more specimens of the *isolated* and perverse examples which " appear at the close of your paper, wherein you go off to new and " separate subjects on every occasion, and by much trying, succeed " at last in catching a single coincidence of a purposely ridiculous " character each time,—do not flatter yourself that you are thereby " reducing to an absurdity, on mathematical principles, the whole " Great Pyramid theory; whose strength consists—not only in the " large number and mutually connected character, of the series of " close coincidences with a united and consilient purpose, which it " proves between that primeval building and nature,—but also, in " the universal earth-importance of the things and system there " symbolized: and I remain, &c. &c. &c."

The gentleman addressed in the preceding letter, has duly returned me an answer, wherein he acknowledges his want of acquaintance with recent Pyramid explorations, so honestly, that I have had nothing else left me to do, than to ask his acceptance of a copy of my "Life and Work at the Great Pyramid;" and the volumes went off to him accordingly, last week.

SIR J. Y. SIMPSON'S PAPER.

So far then for the first of these two essays, in its shortness and simplicity.

Very far otherwise, however, must be the second essay, when it extends, though professedly only an abstract, to no less than twenty-six pages; is written by a home-member and well instructed, of this Society; and is entitled at page 243,—

" *Pyramidal Structures in Egypt and elsewhere, and the objects* " *of their erection.*"

That is the printed title; yet after only two pages on such proposed extensive subject, all the other twenty-four pages are filled up with hardly anything else than severest animadversion upon, merely the one Great Pyramid of Egypt and myself.

I trust that I shall not be accused of conceit for saying in such company *myself*, seeing that in the course of those twenty-four pages my name is printed no less than 85 times; and there are not fewer than 70 cases of extracts in inverted commas, from some one or another of my several Pyramid writings during four years past, besides passages, ideas, facts and numbers more numerous still, similarly derived, but without the commas!

Such a masked battery against one individual, was probably never before opened in the pages of a journal intended for the advancement of science alone; and yet, if its fire had been directed against actual and positive errors, not only might the previous masking of the battery under another title have been condoned, but great praise and extensive thanks would have been due to its engineer—so all-important in the present day of earnest prosecutions of science is it, that the pathways of knowledge should be cleared of every fault, slip, or mistake of each of its previous cultivators. After having, however, carefully read the twenty-four accusatory pages, I am sorry to say, that the general effect on my mind, is not admiration at the clear proofs of error afforded, so much as astonishment at the ingenious and intricate manner in which an *opposite* meaning is given to the majority of those very numerous extracts just alluded to, than what I had intended them to convey.

This fact *may* imply, that my style of writing was imperfect; and perhaps it was; seeing that, like most scientific authors, I wrote for those only who were anxious to understand me. Sad loss of time too would it be, and needless extension of treatises, to attempt anything further; for we see elsewhere, that after the best lawyers of the land have striven to make the wording of some Act of Parliament as exact and stringent as possible, common report tells that other clever men, but of rather perverse inclinations, very soon contrive to find out how to drive a "coach and four horses" through that very, and the self-same, expensive piece of parchment. And if those clever men are further allowed to cut up the one Act of Parliament they are attacking, into quotations of any number of words, from one, to two, three and upwards, and distribute them confusingly among similar shreds of certain abrogated Acts, and other still more extraneous matter also,—you may easily imagine what an erroneous view of the existing law of the land could easily be prepared; but also how totally unusual such a method would be considered among scientific men in the cause of science.

Yet something closely approaching the above has been enacted in the second *Proceedings'* treatise; where little extracts of my last and completest book, are mixed up, generally without distinction or reference, with portions of my earliest, crudest, and now superseded books and papers, on the same subject; and with bits of the late Mr Taylor's book, which bits I never accepted at all.

To prove each and every one of the almost innumerable cases of altered meaning thus produced, would require more time than either the Society is likely to grant, or I have to spare; but I will attempt to separate and exhibit a few, as samples of the rest; and also, point out whatever of new and important matter for our knowledge of the Great Pyramid, the essay appears in my judgment to possess.

LITERARY POINTS.

(1.)

Among more purely literary matters, I would mention first, and with much commendation, the author's reminder at p. 260, of both *Cassini* (in a book published in Amsterdam in 1723), and *Callet* (in Paris in 1795), having proposed "that the Polar axis of the earth should be taken as the standard of measure."

I was not previously aware of their having so done; and have not even yet been able to obtain a sight of Cassini's book.* But a friend in London, who has kindly searched for it at the British Museum, informs me that the statement is not quite exact,—for Cassini does not allude to the *Polar* axis or semi-axis for the purpose, but merely to a general semi-*diameter* of the earth; and he appears to have shared in the scientific uncertainties of most or all men of the period, as to whether the earth is flattened or elongated at the Poles.

The case is interesting, therefore, as setting limits to the date at which *modern* science *could* have begun to single out the Polar axis as a unique length in the earth and appropriate to form a reference for linear measure. But it does not touch the completely independent question in the latter-day theory of the ancient Great Pyramid, as to whether such a standard was known to the primeval designer of that monument 4000 years ago.

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* After having failed in procuring the book at the chief public libraries in Edinburgh, my venerable and learned friend Dr Daun has found it amongst his collection of valuable French mathematical works; and having kindly lent it to me, I can verify the account which follows.

All the parties mentioned do, indeed, propose to divide *a* quantity obtainable from the earth, into ten million parts; but the rod or cubit developed in that manner was not still further proposed to be subdivided by either Cassini or Callet, in the very peculiar manner now attributed to the architect of the Great Pyramid: and his method, if it can be proved, must have had a character, origin, and priority entirely its own.

(2.)

Next, Sir, I have to confess, that when speaking of that ten-millionth part of the earth's Polar radius, as realised for its length only, by a cubit rod of 25 inches ±·1,—when speaking of it in that sense and in some stray sentence, in some one or another of my books, I know not which, nor by what accident the error occurred, —the word *extraordinary* was printed instead of *extraordinarily!*

At least so I gather, because the author of the paper, at his p. 260, drags up the word *extraordinary*, prints it conspicuously in *italics*, puts a *sic* in a parenthesis after it, and publishes that denunciation of his, of what he would indicate as the abominable vulgarity in me, viz., of saying that something was "*extraordinary* convenient;" and he publishes the shocking phrase as a necessary part of his treatise on a scientific subject.

Mr Chairman, and gentlemen,—I acknowledge before you all, that it is a *grievous* fault in polite circles, to use *extraordinary* instead of *extraordinarily*, when the latter is the right word to be used; and my thanks are due to that acute individual who has, it would seem, both detected the error, and made a scare-crow of it before the public. An error light as air perhaps, but admirably adapted on that very account to act as a *wind-vane;* and thereby, even to react on the acute individual, so as to show the world whether the current of his thoughts was setting purely towards the promotion of science for its own sake, when he was writing the remarkable essay now printed in the Society's *Proceedings.*

As to myself, I should, of course, like amazingly to correct this sad error in my books, wherever it exists; but unhappily, no reference being given, I have not been able to find it. I have, indeed, come upon a passage in the 3rd volume (p. 135) of my last book, "Life and Work at the Great Pyramid," identical almost word for word with the passage accused, except in this, that it has not the vulgarism *extraordinary*, but the right word in that place, or *extraordinarily*.

Can the Council assist me in finding where the real sin may lie?

Here is the accusation in their printed *Proceedings*, p. 260.	And here is the passage in my volume iii. p. 135 of "Life and Work at the Great Pyramid," which seems to be the one referred to, but does not contain the evil thing attributed to it:—
"An *extraordinary* (sic) convenient length, too, for man to handle and use in the common affairs of life, is the one ten-millionth of the earth's semi-axis of rotation when it comes to be realised, for it is extremely close to the ordinary human arm, or to the ordinary human pace, in walking with a purpose to measure."	"An extraordinarily convenient length too, for man to handle and use in the common affairs of life, is the one ten-millionth of the earth's semi-axis of rotation, when it comes to be realized (as a linear standard); for it is extremely close, *either to the length of* the ordinary human arm, or to the ordinary human pace, in walking with a purpose to measure."

For the more complete understanding of my version of the original sentence—taken away, as it is here, from the other sentences of its paragraph in its own book—I have inserted, in parenthesis, the words "as a linear standard" after "realized:" because a main purport of the whole is to show, that the modern French metre, being nearly 39 inches in length, is too long; and the ancient Egyptian cubit, only 20·7 inches in length, too short; but the Great Pyramid cubit, being 25 inches long, is most appropriate and handy to use as a measuring rod. And I have called attention above by *italics* to the words "either to the length of," which are found in my book, and assist the above intended meaning, but are omitted in the condemnatory *Proceedings'* edition.

(3.)

Lastly, I would remark under the literary heading, that, in the general description given by the *Proceedings'* author of the present exterior of the Great Pyramid, I am specially quoted for the words

" now it is so injured as to be, in the eyes of some passing travellers " little better than a heap of stones." And the essay then proceeds, in the spirit of that statement, utterly to confound the true figure and peculiar structure of the Great Pyramid, with sundry mere rounded heaps of loose or soft material, sometimes having rude chambers inside them, and sometimes not, in various parts of the world.

It may be quite true, that in one portion of my largest book, where I was anxious to let every side of the question appear in order, the above few quoted words are to be found. But are there not also pages, and pages, and pages giving much broader records of the other side of the question as well; giving my own experience, for instance, through months of patient observation, on the mechanical excellence and conscientious performance of the masonry courses, both composing the Great Pyramid's present faces and general body, and contributing towards one of its most important symbolizations?

How many times, too, have I not had exhibited before large public meetings, both in Edinburgh and various cities of Scotland, amongst other of my own photographs of the Great Pyramid, several which were specially adapted to display that astonishing regularity which extends, with a purpose, throughout each of the gigantic courses of squared, cemented, horizontal, hard masonry? And yet all this accumulated evidence of various kinds, in favour of an ancient excellence, is to be annihilated,—for what? For half a dozen words attributed to, merely "some passing travellers;" and the accounts of passing travellers at the Great Pyramid, are again and again shown, in the same book, to be almost always untrustworthy and liable to gross mistake.

Hence, it is quite clear that the damaging extract to the Great Pyramid, picked out of my book and printed as above, does not exhibit the general sense and total conclusion of my whole book on the particular point concerned, but rather the very opposite. And the superabundant literary acumen which induced a gentleman to make so much use of the word *extraordinary* instead of *extraordinarily* in a minor and rather problematical case, has failed him totally on a grander occasion.

Nor do I see any prospect of that gentleman ever understanding the mode of construction of that noblest work of the world 4000 years ago, viz., the Great Pyramid; and how vastly it differs from all the moats, tumuli, howes and barrows which he will insist on comparing it to, and confusing it with,* until he has himself both ascended and descended the ancient monument, climbing up each of the steep steps of its colossal masonry courses (without any assistance from Arabs or other attendants), and measuring every such step carefully, from bottom to top of the pyramid, and top to bottom; and comparing such collections of measures, made on every face, and every arris line of the structure.

A long and painful process, and perhaps a little dangerous;—but one whose teaching cannot be ignored by any unwilling scholar, so easily as the contents of a printed book.

* As to the total diversity in quality of construction between the Great Pyramid of Egypt, and the little more than mounds of mud and pebble stones forming the barrows of Ireland, frequent testimonies are borne in the volumes of Colonel Howard Vyse—volumes known to the *Proceedings'* author, but seldom quoted by him, when they tend to elevate one's conceptions of the Great Pyramid. As an example of the very important character of some of these omitted witnesses to exceeding perfection of work in one of the earliest, if not the very earliest, of stone buildings now existing, or ever existing, upon the earth; and which has stood there through all the human historic period,—an august witness of what took place under the sun in primeval ages of which we have no other contemporary record, I insert the following from pp. 261 and 262 of the first volume of Colonel Howard Vyse's "Pyramids of Gizeh." The Colonel is speaking of the two casing stones which he discovered by excavating down, through the rubbish accumulated in modern times, to the middle of the north side of the base of the Great Pyramid; and says of them—

"They were quite perfect, had been hewn into the required angle before " they were built in, and had then been polished down to one uniform sur- " face; the joints were scarcely perceptible, and not wider than the thickness " of silver paper; and such is the tenacity of the cement with which they " are held together, that a fragment of one, that has been destroyed, " remained firmly fixed in its original alignement, notwithstanding the lapse " of time and the violence to which it had been exposed. The pavement " beyond the line of the building was well laid, and beautifully finished; but " beneath the edifice it was worked with even greater exactness, and to the " most perfect level, in order, probably, to obtain a lasting foundation for the " magnificent structure to be built upon it. I consider that the workman- " ship displayed in the King's Chamber, in this pavement, and in the casing " stones, is perfectly unrivalled; and there is no reason to doubt that the " whole exterior of this vast structure was covered with the same excellent " masonry."

NUMERICAL POINTS.

The Coffer.

What has now been mentioned, is a prevailing characteristic of the literary parts of this second *Proceedings'* paper; as readers, who can spare the time, will find on tracing up the quotations to the original contexts in my books. More quickly provable matters, however, are alluded to under the headings dealing more particularly with *mensuration* affairs. Therein, too, although the author begins by saying (p. 247), that he approves of the Society having conferred the honour of their Keith Medal upon me, for my measures at the Great Pyramid,—yet within a couple of pages after that, he attacks those measures bitterly, especially as touching the *coffer* in the King's Chamber; and he would make them out to be disgraceful, rather than creditable. For whereas I had cited, as he implies, before going to Egypt, twenty-five different observers,—whose measures of the coffer differed irreconcilably, and by monstrous differences,—yet when I, a twenty-sixth observer, measured it, my observations also differed from every other person's, were apparently no better than any of them, and made a previous confusion only worse confounded.

Now my measures did indeed differ from those of every preceding traveller, in this,—that they were perhaps fifty times more numerous. And, by multiplying them over every part of the coffer,—I was enabled both to show what were the limits of variation in the structure itself;—and also, to prove that all those older recorded measures which differed from certain mean quantities by a whole inch, or even half-an-inch (and some of them varied from the same in any number of inches from two, four, and upwards to so many as forty), were sheer mistakes on the part of the observers: some of whom, it should be stated in apology for them, lived far back in the 16th century, and had guessed the lengths rather than measured them; not having any idea of the importance of extreme accuracy, touching what appeared to them generally,—merely as a burial sarcophagus or coffin.

Irregularities of the Coffer.

This important limitation in old asserted differences of the chief dimensions being thus accomplished,—I next entered into an examination of certain *residual* features, of the coffer.

1. The *first* of these consisted in modern breakages, lamentably large, but sufficiently simple to inquire into.

(They are shown in this model which is $\frac{1}{10}$ the linear size of the real coffer, also in the frontispiece to vol. i. of my "Life and "Work at the Great Pyramid.")

2. The *second* class was caused by traces of a *ledge* cut inside the top of three sides, and all across the fourth side of the coffer.

(In this 2nd model, the breakages being restored, the ledge is exhibited more clearly; they are also shown in the frontispiece plate of vol. i. of above work.)

Now this ledge has a little history connected with it.

Trusting to Professor Greaves, the great French work on Egypt, and other authorities, I had unfortunately described the coffer as without any ledge, *before* I went to Egypt. But on afterwards seeing the coffer there, I found that it *had* traces of a ledge. On my return to this country, I discovered, in a book of thirty years ago,* a notice, short and imperfect, but still a notice, to the same ultimate effect.

On finding these things, I both got that book added to the College Library,—called much attention to it in my own Pyramid book of last year; and published my own observations of the ledge with numerous particulars, both in description and in measure, down to hundredths of inches, in a manner, and with a completeness never before attempted by any one, so far as I am aware.

After all this has been before the public for nearly a year, the *Proceedings*' author republishes at page 252,—and as if elsewhere ascertained—some of my own particulars; and adds to them so much of the defective notice in the old book just mentioned, as to extinguish a very radical difference really existing between the ledge of the coffer in the Great Pyramid, and the ledges on the sarcophagi in all the other Pyramids of Jeezeh. And which difference in form produces this variation in effect.

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* An enormous folio book or portfolio, usually termed *Perring's Plates of the Pyramids*, and containing many excellent lithographs of them from his drawings; but the book was got up, and its letterpress edited by Colonel Howard Vyse, and includes contributions from Dr Birch, Mr Lane, and Mr Andrews as well.

When you put on the cover of one of these smaller pyramid sarcophagi, the combination of *acute-angled* grooves and falling pins fixes and locks the lid, in a manner most suitable to the safe preservation of the contents of a burial sarcophagus or coffin.

But when you put on, to a true model of the restored *coffer* of the Great Pyramid, any sort of sarcophagus lid prepared suitably to its *rectangular* grooves,—such lid has no fixing power, and can be lifted up with the utmost ease; or in a manner very unsuitable to the usual duty of a pure coffin-lid.

Moreover, such a lid seems most untoward to the *coffer* in its place in the King's Chamber; because, while the doorway is of such a height as only *just* to allow the coffer to pass through as a lidless box, or in the state in which every known historian has invariably described it,—put on a lid, prepared in modern times according to the proportions of the lids of sarcophagi of that period, and you can neither get the vessel into, or out of, the room; by an amount too of 5 or 6 inches of solid granite, for that is the space by which the doorway, more than 100 inches thick, is then too low to admit the lidded or sarcophagised coffer.

3. A *third* subject of my minute examinations of the coffer, consisted in certain small residual but original errors or defalcations in its figure, from a pure geometric form; and these are generally so slight, or are effected by curves of such long radius, as to have escaped all my predecessors.

Yet some gentlemen at home, are not always easy to please. And when I supplemented in my printed book some of these additional numbers of mine, by a further verbal warning,—intended for those who are not yet much experienced in the coffer's peculiarities,—the Argus-eyed *Proceedings*' author immediately quotes me, at his p. 251, for the warning, or that, if the sides of the coffer were calipered lower down than the usual place of measuring, they might present a notably different thickness to those who measure to two places of decimals of an inch,—and then adds his own deep insinuation of blame against me, "though it does not appear "why they were not thus calipered."

Pray allow me, Mr Chairman, cheerfully to explain to you why, and how, they both were, and were not, calipered.

They were not *actually* so calipered, because I could not obtain when the idea occurred to me out there, the unusual size of calipers that would have been required, by applying to the scanty resources for civilized life existing in the Libyan desert. But the coffer sides were *virtually* so calipered, by the measures which I took both inside and outside, with such apparatus as I had with me; and any one who is desirous of ascertaining, within small limits, what the sides of the coffer would *actually* caliper at the place indicated, has only to project my printed observations, and see.

CUBIC CONTENTS OF THE COFFER.

My own chief object, however, was not so much to get the thickness of the coffer's sides at one special point, as to ascertain the whole cubical contents of the original vessel.

Now those small *residual* features just mentioned, and which I, by myself, have been chiefly instrumental in bringing to light, having been used by the *Proceedings*' author out of my own pages, for the purpose for calumniating the ancient coffer, and implying (p. 252) that its cavity is "of a form utterly unmeasurable in a correct way "by mere lineal measurement," and pronouncing it to be (p. 254), "in simple truth, nothing more and nothing less than—an old and "somewhat misshapen stone coffin,"—a duty devolves on me to show, both within what limits it can be measured in that lineal way; and also, what remarkable purposes some of those apparent residual errors of figure, but really important adjustments of cubical size, do subserve.

We take the vessel therefore, restored from modern breakages; fill up the ledge in the top of the sides, and then ascertain the cubical contents of the hollow, by multiplying together the mean of all the observed internal lengths, breadths, and depths. From hence we obtain 71,317 cubic Pyramid inches;* a quantity which is many thousands of inches different from the contents of any known sarcophagus of the burial kind at the Pyramids.

When future observers shall have visited the Great Pyramid, and measured the coffer with still more care than I have done,—their results will afford a desirable test as to my limits of error. But meanwhile the coffer itself may testify something, by having (when its ledge is filled up, and only then) a certain inter-commensurability of parts; and which is not known to prevail in any Egyptian sarcophagus, or to be now, or ever to have been, a necessary principle amongst makers of mere coffins anywhere.

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* Mean length = 77·85; mean breadth = 26·70; and mean height = 34·31 Pyramid inches. See "Life and Work," vol. iii. p. 154.

The first of these commensurabilities is,—that within the limits of errors of measure, the inside contents of the vessel are just half those of the outside (as per model shown at the meeting).

And the second is, that the cubical contents of the bottom are similarly half those of the sides (as per model).

But these distinct proportions would not have obtained within many hundreds of cubic inches, if the recently detected hollowing of some of the sides of the coffer had not been performed of old. So much pains do indeed seem to have been taken in that matter at the primeval coffer, that we may be justified in adding beneath our first quantity in the numerical table, half the observed contents of the outside of the coffer, = 71,160;* and the sum of the walls and floor, obtained chiefly by separate measures, and = 71,266, on a principle discovered by Mr Henry Perigal.

These results are beginning to look close: and although the *Proceedings*' author would vitiate them utterly, by insinuating that I have measured them from what he stigmatises as an "imaginary "higher brim" (p. 252),—yet I point on this plate of my published book, "Life and Work" (vol. i. frontispiece), and also on this model, to the still remaining traces of the actual and ancient top

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* TABLE of COFFER—

Measures above referred to, all expressed in Pyramid inches.

Number of Method.	Nature of Method.	Numerical materials employed.	Cubic Contents of Interior of Coffer.
1	By direct measures of interior,	$77{\cdot}85 \times 26{\cdot}70 \times 34{\cdot}31 =$	71,317·
2	By half the exterior measures,	$\dfrac{89{\cdot}62 \times 38{\cdot}61 \times 41{\cdot}13}{2} =$	71,160·
3	By contents of bottom and sides,	$89{\cdot}62 \times 38{\cdot}61 \times 6{\cdot}866 = 23{,}758\cdot$ $2\,(89{\cdot}62 + 26{\cdot}70) \times 34{\cdot}31 \times 5{\cdot}952 = 47{,}508\cdot$	71,266·
4	By new depth derived from ledge frame,	$77{\cdot}85 \times 26{\cdot}70 \times 34{\cdot}282 =$	71,258·
		Mean =	71,250·

of the sides: from which my measures were scrupulously taken. Indeed I repel that word *imaginary*, and fling it back to its author; and declare that those traces of the original top of the sides were to the best of my testing and examination as unmistakably ancient, finished, and precise as any part of the coffer whatever.

I have also been led since then to perceive, that the frame adapted to fill up the ledge, seems to be an independent confirmation of the sides' original height. It is so, at least, within less than three hundredths of an inch;* and that difference being introduced into our first computation, gives us a fourth approach to the true cubical contents of the coffer = 71,258.

These four being combined, give 71,250 cubic Pyramid inches, as the best result of all my measurings, true probably, for the coffer itself, to within twenty cubic inches. And I venture to say this, notwithstanding that the author in the *Proceedings* attacks me violently by means of some of the old and exploded twenty-five observers of former times, setting forth (at p. 250) that one of their results differs by 6000 cubic inches, and another by no less than 14,000 cubic inches, from mine.

But Sir, is that to my, or to their, confusion?

If you will examine the particulars, you will find that the 6000 inches of difference, arose from that party having made an absolute error of 3 whole inches in the depth of the coffer.† While the 14,000 inches case rests on one Dr Whitman, who himself never went inside the Great Pyramid, but depended on a friend.

If the author in your *Proceedings* will think that Dr Whitman could understand the coffer better without going near it,

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* For full numerical particulars want of space here compels me to refer to a work which I have now almost ready for publication, and entitled, "On the Antiquity of Intellectual Man." *See* its chapter 29, p. 300.

† The faulty measure was that of the French Academicians in 1799, and when reduced from the metre to British inches, = 37·285.

Professor Greaves, in 1638, had previously stated the depth = 34·320 British inches; and Colonel Howard Vyse had subsequently, or in 1837, made it = 34·5 British inches. But still many persons thought, "surely " the French Academicians could not have made so great a mistake in their " measurings; it must be the fault of the coffer, whose depth is different in " different parts of its length or breadth?"

The following measures, however, taken by myself in 1865, will show clearly that though the depth may vary over different parts of the bottom by or seeing it, than I could by spending many days over it,—who can prevent him? I certainly cannot, and do not intend to try. I have published the particulars of my modes of measurement, the nature of the apparatus employed, the method of comparing with a standard, the circumstances under which the observations were made, and the actual numbers procured from the measurings and remeasurings arranged so as to test each other on various parts of the coffer; and if all these particulars fail to impress the gentleman's mind, as of some weight or importance in a case where next to nothing can be said on the opposite side, I can offer no more to him either in explanation or defence of myself.

But before the world, I must still continue to maintain, that so far as both care and number are concerned, no coffer measures have yet appeared that can compare, on the usual principles for judging measures amongst scientific men, with mine; that mine, therefore, however much below ideal perfection, have a practical claim to represent the true size of the coffer, until better observations shall be taken; and that, if distressingly bad measures, or mere guesses at measures, have been published during times long past—their badness is the fault of the observers, not of the coffer.* Yet, to

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hundredths, and occasionally even by tenths, of inches,—a whole inch is perfectly out of the question.

Extract from "*Life and Work,*" vol. ii. p. 123.

"INSIDE DEPTH of *Coffer*, from original top of North, East, and South Sides. (By Slider 25, not requiring any correction.)

Part of Length where Observations were taken.	Part of Breadth where Observations were taken.			
	Near East. Side.	Near Middle.	Near West Side.	Mean at each part of Length.
British inches.	B. inches.	B. inches.	B. inches.	B. inches.
0·6 south of inner N. end.	34·30	34·28	34·26	34·28
3·0 " "	34·44	34·36	34·35	34·38
5·0 " "	34·42	34·41	34·28	34·37
10·0 " "	34·40	34·38	34·28	34·35
24·0 " "	34·36	34·38	34·26	34·33
Mean at each part of Breadth,	34·38	34·36	34·29	34·34
General Mean of all the Measures, or Mean *Depth* of Coffer, inside, . .	= 34·34 British inches."			

* For cause of error in Dr Whitman's numbers, see note on p. 338.

attribute all the fault, and prove it heinous, to the coffer, whose makers and friends have been dead for 4000 years, would seem to be the very object of the *Proceedings*' author; in order that he may then take away the primeval vessel's power to symbolize a standard of measure. For says he at p. 249, " Surely a measure of capacity should be measurable; " that ought to be its most unquestionable " quality; but this imagined standard has proved virtually unmeasur- " able,—in so far at least as its twenty-six different and skilled " measurers all differ from each other in respect to its dimensions."

And as the same author repeats, at his page 252, that " the coffer's " cavity seems really of a form utterly unmeasurable in a correct " way by mere lineal measurement,"—and indicates that though " perhaps liquid measurements would be more successful,"* yet the object is not " of the slightest moment;"—it must now appear plainly, that the grand purport of that gentleman's essay is not to ascertain more correctly than I have done, or attempted to do, what is the real capacity of the interior of the coffer, nor to furnish the world with correcter numbers than mine,—but merely to damage the ancient vessel's character, as to being considered capable of showing any particular capacity, even within 14,000 cubic inches.

He may argue that this is a mode of inquiring legitimately into whether the Pyramid can be possibly regarded as a metrological monument, or a sepulchre (p. 247). But seeing that the style of workmanship of the whole king's chamber, as well as of the coffer that stands in it, has for ages excited the admiration of all nations, for the truthful character of its rectangular shapes, polished surfaces, and close fitting joints,—seeing, I say, that such good and simple mechanical work in a hard material is precisely what admits being well measured by competent men,—and that Professor Greave's length for the whole room 200 years ago, differed from my measure of the same, by less than $\frac{1}{40,000}$ part; why the *Proceedings*' author, if he *will* maintain the " skilled " and trustworthy measures of *all* the twenty-five observers,—would imply that the hollow granite block forming the coffer, and reported by those observers in different years, as of various sizes,—although it is resting quietly in a dark room of equal temperature,—must actually, with time, alter its size :—alter it too so hugely, that the granite swells out sometimes to a length of 144 inches, sometimes sinks down to 78 inches only, while still preserving the same general rectangular box-like figure.

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* How such measure is to be applied to a vessel broken down at one corner to more than a third of its height, the *Proceedings*' author does not say.

Whether other persons may be inclined to maintain that hard red granite *can* undergo such changes, I do not stop to inquire. I only vouch that I saw no inclinations to it, on comparing my measures, taken at the beginning of my stay at the Great Pyramid, or in January, with my last on leaving it in April. Moreover I firmly believe that any or all my twenty-five predecessors, if,—instead of, as with the generality of them, remaining in the coffer room only a few minutes on a single occasion,—if they had visited it day after day, for hours each time, and during several months of the year,—and if they had taken the same pains that I did, to compare their measuring scales accurately with the Government standard yard of 36 British inches,—they would have very nearly brought out as their result for the coffer's cubic contents (before breakage had been effected or a ledge had been cut into it), the same quantity which I have found, viz., 71,250 cubic pyramid inches;* and that equally, whether they look on the coffer as a burial sarcophagus only, or a a measure of capacity, or both combined.

If *I* consider the coffer chiefly as a measure of capacity,—though freely confessing that some subsidiary depositing of a mummied corpse in state, may once have been performed there,—it is because, after having found that the amount of cubic space, both in length, breadth, but more especially in depth, is much greater than would have been positively required for a mere coffin purpose,—considerably greater too, in depth, than in the manifestly burial sarcophagi of other pyramids,—it yet tallies exactly with an expression deducible, —on what has otherwise been called "Pyramid principles,"—from the size and weight of the whole earth, for appropriately representing a grand standard of capacity, and weight, measures. The very essence of the question turns, in fact, on whether there be a neat and close correspondence between the one practically measured, and this other theoretically computed, quantity.

What then has not been my surprise to find the *Proceedings'* author, positively giving as my theoretical reason for the coffer having been made in ancient days the size it now proves to be, not the noble, appropriate, and earth-reference reason which I have published,—but something totally opposite, chronologically foolish beyond expression, and which, so far as I know, never either entered my head or escaped my lips on any occasion whatever.

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* A pyramid linear inch = 1·001 British inch.

I subjoin the two statements, in order that the Council of the Society may judge for themselves :—

Proceedings' paper, p. 249.	"*Life and Work*," vol. iii. p. 151.
"Professor Smyth holds that *theoretically* its (the coffer's) capacity ought to be 71,250 'pyramidal' cubic inches, for that cubic size would make it the exact measure for a chaldron, or practically the vessel would then contain exactly four quarters of wheat."	"A cubic space is to be formed, with sides having a length equal to one ten-millionth of the earth's axis of rotation, or 50 Pyramid inches. A *tenth* part of such space, or 12,500 cubic inches (agreeably with the Coptic interpretation of the name of Pyramid), is then to be filled with matter of the mean-density, or specific gravity of the earth as a whole. In which case, such a mass will form the grand *weight standard* of the Pyramid ; while the *space* occupied by an equal weight of pure water at a given temperature, will form the grand *capacity standard* of the Great Pyramid ; or, as we believe, will represent, and be represented by, the cubic contents of the hollow of the coffer. And further on it is shown, that the mean density of the earth is nearly 5·7 times that of water at a temperature of 68° Fahr. Whence the theoretical expression for the coffer's capacity is, $\frac{50^3 \times 5{\cdot}7}{10} = 71{,}250$, Pyramid cubic inches.

Arabian Authorities.

There is only one more circumstance which I have time to notice, connected with this *Proceedings*' author's account of the coffer, but *that* one is said to have procured him many believers.

It is, that the Arabian writer, Al Hokm, states that Khaliph Al Mamoon, when he broke into the Great Pyramid, in or near to 830 A.D., found in the coffer an embalmed human corpse; proving, therefore, as some will have it, that the coffer had been from the beginning *only a coffin.* Wherefore, let us inquire, who is this Al Hokm, who vouches, and is so readily accepted as a voucher, that Al Mamoon's chief finding was a dead body?

Mr St John Day showed two months ago, before the Philosophical Society of Glasgow, on the strength of the publications of Colonel Howard Vyse and Dr Sprenger, that Al Hokm lived 600 years after Al Mamoon; and could not, therefore, be any certain or *contemporary* authority for what that Khaliph found in a dark chamber in the Great Pyramid, six centuries before. Mr Day also showed, out of Vyse's book, that there is no known Arab writer who begins to attribute body-finding to Al Mamoon, within the first three centuries after his death; those who are earlier, and closer to that Khaliph's times, having completely different stories to tell; and telling them too in the most positive manner.

Yet the *Proceedings'* author will by no means give up his selected Mohammedan latter-day tales (or those which Mr Day had criticised in their newspaper edition of January 22, 1868); and after adducing, in a note, two other Arab writers, to a similar effect with Al Hokm,—but both of them living between 300 and 400 years after Al Mamoon, according to the same either Howard Vyse's or Dr Sprenger's authority,—he virtually clenches up these later Arab accounts on to, or makes them as good as if written in, Al Mamoon's own day, by stating, most remarkably, at the end of the same note,—that "*Colonel Vyse observes, that the Arabian*
"*authors have given the same accounts of the pyramids, with little or*
"*no variation, for above a thousand years.*"

Yet neither the words, nor the sentiments in the above juxtaposition, are Colonel Howard Vyse's. The words are out of his book, no doubt, but they are spoken by Dr Sprenger; and, what is much more important, he is applying them to authors of far earlier date than any of those who mentioned Al Mamoon.

That it was to these far earlier men, talking of perfectly different matters, that Dr Sprenger was alluding,—would have appeared clearly enough in the *Proceedings* and even from the very sentence which the *Proceedings'* author has quoted,—had not this gentleman cut off the last half of it.

Here, Sir, accordingly, from the note to *Proceedings*, page 253, where the first half of Dr Sprenger's sentence is made to corroborate statements which the Dr never intended it to be seen in company with,—is that which is quoted there, or thus—

" 'It may be remarked,' observes Colonel Vyse, 'that the " Arabian authors have given the same accounts of the pyramids, " with little or no variation, for above a thousand years.' (Vol. ii. " p. 328.)"

And here, from that page of Howard Vyse's book, is the full sentence, as it appears in the whole note which has Dr Sprenger's name conspicuously subscribed to it:—

" It may be remarked that the Arabian authors have given the " same accounts of the Pyramids, with little or no variation, for " above a thousand years; and that they appear to have repeated " the traditions of the ancient Egyptians, mixed up with fabulous " stories and incidents, certainly not of Mahometan invention."

In fact, the main purpose of the note is to explain what were those unvaried stories of a thousand years; and it is attached by Dr Sprenger directly to the account of one *Masoudi*, who, living within 140 years of *Al Mamoon*, mentions him not, but abounds in histories of the wonderful circumstances attending the building of the Pyramids by certain kings before the Flood; and also of the magical treasures and enchanted wonders which had been found in the monuments in recent times.

Neither Dr Sprenger himself, therefore, nor Colonel Howard Vyse could have intended the sentence to apply, either partially or wholly, to new contents of the latter's subsequent pages, 351, 352; where he introduces, in his chronological arrangement of Arab writers,* the *Proceedings'* author's Al Mamoon authority, Ebn Abd Al Hokm, between Firazabadi, who died in 1438 A.D., and Makrizi, who died in 1467 A.D. Nor to his pages 333 and 355, where he mentions the other two Al Mamoon-noticing writers of the *Pro-*

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* " The Arabic authorities have been translated by Dr Sprenger, and I " have endeavoured to arrange them chronologically; a task which has been " attended with some difficulty, as many of them are only known by quota- " tions in the works of posterior writers."—Colonel Howard Vyse's *Pyramids of Gizeh*, vol. ii. p. 179.

ceedings' author, viz., Alkaisi, died in 1187 A.D., and Abou Szalt, of Spain, possibly of about 1400 A.D.

(Hence the attempt made in the *Proceedings* note to p. 253, to produce out of Vyse's books an affirmative testimony to the contemporary character of Al Hokm's relation touching the Khaliph Al Mamoon's reported discovery of a body in the coffer,—entirely breaks down. And as it is of very little consequence for identification with the original builders of the Great Pyramid and their intentions or work, *what* the Khaliph found in the coffer;—seeing that the Pyramid is now considered to have been entered forcibly by men of alien faith, some 1500 years after it was built, but more than 1400 years before the time of Al Mamoon,—we might leave the matter now at rest, but for an additional statement made by the *Proceedings*' author before the Royal Society on April 20. For on that occasion he announced having received a letter from the British Museum, in effect completely ignoring his former authorities, Col. Howard Vyse and Dr Sprenger; and affirming that Al Hakm lived contemporarily with the Khaliph Al Mamoon.

Whether that letter is to be regarded as throwing every other authority into the shade, I do not pretend to know; but the whole document should now be printed and submitted to the same public opinion which has so long sat approvingly on Col. Howard Vyse's and Dr Sprenger's opposite and fuller statements).*†

SIZE OF THE BASE OF THE GREAT PYRAMID.

After stating in his page 254, in a singularly objectionable manner, some of my reasons for considering the determination of the

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* Added to the original paper after the meeting.

† This is the note referred to at foot of page 332, respecting Dr Whitman's numbers for the cubic contents of the coffer in the Great Pyramid.

Having already shown in the note to pages 331, 332, how the 6000 cubic inches, case of difference, from my *coffer* measures, is explainable by the former observer, M. Jomard, having without doubt made an absolute error (probably in copying his notes) of 3 whole inches in the depth of the coffer; (for these 3 inches being subtracted before the multiplications are performed, the alleged difference nearly vanishes):—I have now to show that the other alleged case of a difference, or that under the name of Dr Whitman, and to the horrifying extent of 14,000 cubic inches, depends mainly on a blunder of still more transparent character.

Its component numbers are published in Howard Vyse's second volume, length of the sides of the base of the Great Pyramid to be a most important problem in a metrological point of view,—the *Proceedings*' author thus addresses himself *apparently* to that question, under the express title of "What, then, is the exact length of one of its "basis lines?" but *really* to the purpose of blaming me for nearly everything which I either have done, or have not done, with regard to both computations and measurements in the matter.

Apparently for the first purpose, I say, because I cannot find, after going through all his pages, that he has advanced the question beyond the point where I left it, or come to any other distinct conclusion, or discovered any new authorities. But *really* for the second purpose, from what follows: though I shall only touch upon such of the numerous insinuations as may tend, in their

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p. 287, as given to Dr Whitman by a British officer of Engineers, and appear there as follows:—

		Feet.	Inches.
Sarcophagus......	Length,	6	6
„	Height,	3	5½
„	Thickness of Stone,	0	6
„	Width within side,	2	2¾
„	Depth, ditto,	2	8

The general aspect of these measures, taken, as they are, either to whole inches only, or mere halves and quarters, shows that no great accuracy was aimed at by the said engineer officer. We may also conclude similarly from the thickness of sides, ends, and bottom being all indiscriminately lumped together as "thickness of stone;" especially when we find that the difference of height (outside measure evidently from the term) and depth (inside measure also evidently from the term, and from its likewise being expressly so stated) makes the bottom thicker by more than one-half of the previously stated general thickness of anything and everything about the coffer.

But the chief anomaly touches the *Length*. That is given only once, and without any direct statement of whether it applies to outside or inside of the vessel; while there is the indirect symptom that it means outside measure, from its standing immediately above "*Height*," which is a confessed outside reference, and as far as possible from depth and width, both stated to be inside. Hence, looking to the given measures-list, *per se*, we can only take the length given there, or 78 inches, as outside measure,—and when we subtract from it double the "thickness of stone," there result 66 inches for the *inside* length; and that quantity used with the given inside width and depth does undoubtedly give a capacity content, smaller than my determination by about 14,000 inches.

That 66 inches, however, for inside length, is close upon a foot smaller than my measure, which is supported within a very small fraction of an inch explanations, to throw some useful light on either the proportions of the Great Pyramid, or the principles of that more careful mensuration which should be applied to it.

General Principles of Accurate Linear Mensuration.

In order to measure, then, any length of earth lines, in the accurate manner of the best base-line operations of trigonometrical surveys—in which I have myself, in former years, borne a practical part—and which are a very fair ideal of accuracy to look towards in Great Pyramid outside measurements,—it is necessary

1*st*, To have well-defined and fixed terminal points at either end of the line to be measured.

2*d*, To have the ground tolerably well cleared, levelled, or reduced to gentle gradients, between the points. And,

3*d*, To have scientific apparatus, by which the measuring bars may be placed in definite linear positions, and to microscopic accuracy both absolutely level and parallel to the line joining the two terminal points; under temperature circumstances, also, where the deviation of length of the measuring bars, from *a chosen standard, shall be accurately computable.*

by Col. Howard Vyse, the French Academicians, Prof. Greaves, and many others. Wherefore arise the following questions:—

(1.) Was the coffer really of a different length when Dr Whitman's engineer visited it, than in the times of the other measurers alluded to?

(2.) Supposing that the above was not the case, then, A, did the engineer officer make a mistake in his measures themselves, to the extent of 1 foot in 6 feet? or, B, did he, or perhaps Dr Whitman, merely misplace in his notes what he had measured fairly as inside measure, and place it amongst outside measures heedlessly?

I incline, now that Dr Whitman's published numbers have been dragged up as being a very high authority on the coffer's size, to choose supposition B as being the most probable. And then have to take for his *Length* inside, 78 inches; *Breadth* inside, 26·75 inches; and for *Depth* inside, 32 inches by direct measure, and 35·5 if we subtract his "thickness of stone" from his outside *Height*, the mean of these two being 33·75 inches.

Reducing these values from British inches to Pyramid inches, we have 77·92 × 26·72 × 33·72 = 70,206 Pyramid cubic inches, where the alleged 14,000 inches of difference are reduced to nearly 1000 inches; and even that is chiefly chargeable on evident rudeness and mistakes in the *Depth* measure; for the true quantity (say 34·31), is included within the two very wide determinations given.

These Principles applied to the higher Surveying.

Requirements 1 and 2 are comparatively simple to most geodesists; for these gentlemen purposely choose out a plain already nearly level, and insert at either end of the line they select therein, a defining mark of their own, say a block of stone,—plugged near the middle of its upper surface with a brass bolt, and that again with a platinum stud, and the platinum further marked with a microscopic black dot in its centre. Then the horizontal distance from the minute dot at one end of the line, say from 3 to 8 miles long, to the similar minute dot at the other end of that line, is the practical problem to be solved by the measurers, through employment of what we have indicated under head No. 3.

But then comes the grand difficulty to them; and it would be fearful to tell the number of tens of thousands of pounds which the Ordnance Survey of this country has expended, both directly and indirectly, on various methods of base-line measuring instruments, from General Roy's wooden bars, glass bars, and steel chains, up to Gen. Colby's compound compensation bars, with their microscopic and virtual, not actual, contacts. This last description of apparatus is now allowed to be very near perfection when properly worked; but to work it as it should be, requires amounts of officers, non-commissioned officers, men, tents, camp-equipage, measuring bars, microscopes, bar-comparing stations, lining theodolites, adjusting screw tripods, wooden stands, ground-clearing implements, sun-warding means, &c., &c.,—which I believe even still bring up the cost of a base line, either on the Indian or the British Trigonometrical Survey, to several thousand pounds. Yet, whatever the expense, it is cheerfully incurred there; because the accurate length of the line is required to be known, and it cannot be ascertained at a less cost of labour, instruments, and money.

How the Principles should be applied to Pyramid-base Mensuration.

Next we have to apply the same acknowledged principles to the mensuration of the Great Pyramid, or rather, as at present the case is presented to us, the four sides of its base.

Under head No. 1, *defining points* exist there already in the shape of the outer corners of the sockets cut anciently into the solid rock of the hill, to receive the corner-stones of the exterior casing of the Pyramid. These sockets are easy to find and uncover, as I have described in "Life and Work," vol. i. ch. 17; but they cannot be fully depended on for being such, until they shall have been checked by comparing them with traces, such as Colonel Howard Vyse discovered in 1837 on the north side of the Pyramid, in the shape of some of the casing-stones forming the middle of that base-side of the Pyramid, and still *in situ*, or firmly attached to the rock, or lower pavement; (see note to p. 325.)

Equally therefore to test the sockets found, being the right sockets, and also to have the ground between them fit for employing accurate measuring instruments upon,—must attention be paid, to realizing our head No. 2, at the Great Pyramid. There, indeed, this No. 2 becomes the most necessary beginning, and the most expensive part of the whole operation. For every side of the base is at present encumbered by such huge hills of compacted stone rubbish, that no accurate mensuration can be performed over them, and they completely hide under their substance all the fiducial traces of ancient workmanship which we require to get at; and without getting at which we cannot tell whether what we are measuring was once the base-side of thePyramid, or perhaps something else.

If these requirements of No. 2, should on some occasion be somehow or other performed, then No. 3 must be put into execution with all the niceties, and abundance of instruments and men described under the same head for an ordinary trigonometrical survey base-line. The distance indeed to be measured would be much less, but the attention of intelligent and educated men would require to be exerted in an original manner on the marks and meanings left in the lower parts of the Pyramid, the pavement surrounding it, and the rock on which it was founded by its builders of 4000 years ago,—in a manner and degree never exacted from Ordnance officers and men when merely measuring a base-line in a natural plain, for purposes of modern science alone.

Hence I am rather disposed to make the rough estimate, that if No. 1 at the Great Pyramid could be accomplished by an expenditure of L.10, No. 2 would require L.7000, and No. 3 L.3000; and the fully accurate mensuration of the Great Pyramid will never be realised until all these three heads have been carried out by competent parties, and *simultaneously*.

Failure of efforts to have the Principles applied in 1864–5.

I need hardly say, therefore, that when I went out to Egypt in 1864, at my own private expense, and on the savings of a salary of L.300 per annum, I did not contemplate, of myself or unaided, measuring the base-sides of the Great Pyramid at an expense of L.10,000. Knowing, however, from my former experience on a trigonometrical survey, how the scientific part of the operations ought to be performed, I did all I could, before going to the Great Pyramid from Cairo, to urge the adoption of the necessary measures both on His Highness the Viceroy of Egypt and on Egyptian Society. But no one would look at the proposal. The expense of carrying out the preliminary head No. 2, frightened them all. They remembered that even Colonel Howard Vyse, who spent his money *en prince*, had only made three crosscuts *into* the rubbish-mound on the north side of the Pyramid's base, and had not attempted the far greater work of cutting through the *length* if it; while the similar mounds on the other three sides have remained unattempted in any way by any one for the last 700 years at least. And if no one in Cairo would undertake this operation No. 2, of course it was futile to ask them to enter into head No. 3; while it was likely to lead to premature and irreparable mischief being done to the fiducial points of the Pyramid, to ask them to undertake, or sanction my undertaking, what is implied under head No. 1, by itself. I had, therefore, to go out to the Great Pyramid unaided for that work; leave the proper measurement of its base-sides as a legacy to richer men and future times; and address myself to what I could do, alone, with my own hands and very modest apparatus. That accordingly formed my daily toil * from early morning to late at evening, during the whole four months I was at the Pyramid; unassisted all that time by any other measurer, until the very last few days, when a new party appeared on the scene, and occupied thereafter the place which—my time and means being then just exhausted,—I was shortly after obliged to vacate.

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* Since published in the 1st and 2d volumes of "Life and Work at the Great Pyramid."

Libel the First.

The apparition of this new party, however, has furnished the *Proceedings'* author with an opportunity for serious misstatements and grave insinuations, which he has availed himself of in the following manner in p. 255 :—

"At the time at which Professor Smyth was living at the "Pyramid, Mr Inglis, of Glasgow, visited it, and, for correct "measurement, laid bare for the first time the four corner sockets. "Mr Inglis' measurements not only differed from all the other "measurements of *one side* base-lines made before him, but he "makes the four sides differ from each other; one of them—"namely, the north side—being longer than the other three. "Strangely, Professor Smyth, though in Egypt for the purpose of "measuring the different parts of the pyramid,—and holding that "its base-line ought to be our grand standard of measure,—and "further holding that the base-line could only be accurately ascertained by measuring from socket to socket—never attempted "that linear measurement himself after the sockets were cleared. "These four corner sockets were never exposed before in historic "times; and it may be very long before an opportunity of seeing "and using them again shall ever be afforded to any other "measurers."

This paragraph plainly begins with implying, that *at*, or during, the time I was living at the Pyramid, Mr Inglis (a young and worthy, but poor, engineer's assistant) visited it, made all the preparations necessary for correct measure of the sides of the base; measured them, got into some anomaly about the results; and was not assisted by me, who, although there for the express purpose of performing such a measure, yet "*strangely*" did nothing; and have caused an opportunity to be lost, the like of which never occurred before within history, and may be very long before it will occur again. Wherefore, some of our members may remark, "is that "the person to whom the Society should have given their Keith "medal for his *measures at the Great Pyramid!*"

But I will now set forth how vastly different from the truth, is the above statement of the *Proceedings'* author.

1. Mr Inglis' visit was not made *during* my stay at the Great Pyramid, but close to the latter end of, and after, it. He arrived, when I was, in fact, preparing to leave, had many of my instruments already packed up for removal, and had even fixed a day with the Egyptian government for removing, and had to keep to that day; and he not only outstayed me, but occupied my rooms after I had left, and was much more my successor than contemporary at the Great Pyramid.

2. Mr Inglis did not visit the Great Pyramid on his own scanty means, as I did on mine; or for his own purposes: he was sent there, paid for being there, and furnished with funds for engaging assistants by his employer, Mr Aiton, a weathy contractor with a hundred times the amount of money that I had. A very fearless and independent man too, this Mr Aiton—who had obtained from the Egyptian government greater powers for exploration than I had received; held his own ideas about the Great Pyramid, and amongst others—as rumour said—wished to upset everything that I was doing or concluding about, touching the ancient monument. And I do believe, from all that I have seen since, that if Mr Aiton's employé who stepped into my place as above described, when I left the Pyramid in April 1865, had found anything materially wrong in my account of the same, Mr Aiton was precisely the man to let all the world know of it, and even ring with it, long before this. While, if he *would* open the sockets, and expose them thereby to the chance of irreparable mischief, *he* was the person morally bound to have them properly measured.

3. But Mr Inglis, with the Arabs whom he engaged with Mr Aiton's money, did not by any means do everything that was required for the correct measure of the sides of the Pyramid base. Indeed, they only executed No. 1—the least expensive of the *three* necessary heads which I have noted under p. 340. And even one half of that No. 1, they did not achieve without my assistance, as narrated in "Life and Work," vol. i. p. 532–535.

4. When all the four corner sockets were thus imperfectly opened up, Mr Inglis and myself divided between us such observations as it seemed possible then to make; he chose the *linear*

because he had some tolerable linear, but *no angular*, apparatus:—and I freely left him with the linear duty, which I foresaw could only be approximate, and, in so far, could be as well done by him as by me—while I undertook the angular work; and which, if I had not undertaken, then and there, would not have been attended to at all, on that occasion of the sockets being opened up. Moreover, my time had become so limited that I could not have undertaken both species of measurement; nay, even with the angular measures alone, I was not only occupied with them from the time of the sockets being discovered, to the moment of my being removed from the Pyramid by the Egyptian government,—and then had to leave some of those angular quantities very imperfectly observed and some not at all,—but Mr Inglis kindly assisted me in what I was enabled to accomplish; as fully described and gratefully acknowledged in "Life and Work," vol. i. And it was not until after I had left the Pyramid altogether, that Mr Inglis found time to complete his promised *linear* part of the measure, or the lengths, and levellings, from socket to socket. He laboured at them, as I believe, very conscientiously, and sent me the results by a special messenger when I was leaving Egypt for Scotland: and by me, after having obtained Mr Aiton's consent and approval, they have been published to the world in the name of that gentleman and his assistant.

What credit there is, therefore, in those linear measures—the first that had ever been taken from socket to socket *round all four* of the sides of the Pyramid—belongs, worthily to Messrs Aiton and Inglis, and no one can take it from them. But what discredit is now sought to be attached to them by a certain party, because their measures are not of the exalted accuracy of a well-measured trigonometrical survey base-line, is not theirs (Messrs Aiton and Inglis)—much less is it mine, as the *Proceedings*' author tries to insinuate—but it is the necessary consequence, which I expected from the first, of the measurement being attempted on the strength of No. 1 only having been performed, and no attention paid to Nos. 2 and 3 both of my p. 340, and of my representation, from the first moment of arriving in Egypt, to His Highness the Viceroy.

Libel Second.

Yet, Sir, however far from the accuracy compassable by employing methods 1, 2, and 3, *simultaneously*, may be Messrs Aiton and Inglis' measures, made on the strength of method 1 alone,—they are vastly above anything that can be done in the present ruinous and rubbish encumbered state of the outside of the Great Pyramid, *without* the assistance of that No. 1.

After once, therefore, having obtained, from any observers, any *socket* measures of the base-sides, I have deemed it waste of time to try to get the true base-side length from any older measures taken by observers who had no view of the sockets to guide them; and I have condemned my own attempts as well as those of others irretrievably, when taken under such impossible circumstances for accuracy. Those ante-socket measures were well enough in their day, as the only approximations then procurable; but their day closed the moment that the vastly better approximations, caused by any sort of measurings from socket to socket, had been obtained.

Not a little surprised, therefore, am I to find in a paragraph extending from p. 255 to p. 256, that the *Proceedings*' author, after having begun to deal with *socket* measures, goes back to some ante-socket measures, or the rudest possible approximation of mine, taken merely to check a rumour that two of the Pyramid basis sides were 1000 or 2000 inches longer than the other two; and he enters into a long argument as to whether I am not egregiously wrong in applying to them a correction for a double thickness of casing-stones, less by the comparatively insignificant quantity of 14 inches, than what *he* would have applied.

I shall not go into that argument at all on the present occasion, or attempt to refute one chief insinuation involved, viz., that that rude approximation is a sample of what my best measuring for final purposes would be; because—*first*, I have allowed that my rude observation to be corrected, may be in itself erroneous by *many times* the amount of 14 inches; *second*, I have nothing final founded on it, having positively rejected the whole affair, observation, corrections, and all, as unworthy of being looked to for a moment, as unworthy of "any attention" for the theory, since Messrs Aiton and Inglis' *socket* observations were procured: as may be seen in my "Life and Work," vol. iii. pp. 123 and 124. And *third*, I still think that the course I followed was the correct one to be pursued under the circumstances then existing.*

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* Although I would not interfere with the current of the argument in the above pages, concerning the socket measures for the lengths of the base-sides of the Great Pyramid, by introducing there any refutations of charges brought against me on a different matter by the *Proceedings*' author,—I had no intention of eluding altogether any Pyramid accusation by him, which implies faults of a most serious nature.

Such an accusation is this (pp. 255 and 256)—"But Professor Smyth has "'elected' (to use his own expression) not to take the mathematically exact "measure of the casing stones as given by Colonel Vyse and Mr Perring, "who alone ever saw them and measured them (for they were destroyed "shortly after their discovery in 1837), but to take them, without any "adequate reason, and contrary to their mathematical measurement, as equal "only to 202 inches,———."

This passage evidently implies that I had taken as 202 inches only, what a mathematically accurate measure had made something more; how much then? In the previous two sentences of the *Proceedings*' author (p. 255) we read that Professor Smyth "made each side of the present masonry courses "(of the base of the Great Pyramid) 'between 8900 and 9000 inches in "length,' or (to use his own word) '*about*' 8950 inches for the mean length "of one of the four sides of the base; exclusive of the ancient casing and "backing stones—which last Colonel Howard Vyse found and measured to "be precisely 108 inches on each side, or 216 on both sides. These 216 "inches, added to Professor Smyth's measure of 'about' 8950 inches, makes "one side (of the base of the Pyramid) 9166 inches."

Here then, evidently, we may see that what the *Proceedings*' author attaches 216 to, is not the casing stones alone, as mentioned in the sentence previously quoted; but, either the backing stones by themselves, or backing stones with casing stones; yet whichever it was, he plainly says that Colonel Howard Vyse measured the quantity *to be precisely* 108 *inches on each side, or* 216 *on both sides;* and we must presume that this is the *mathematically exact measure* which he would have had Professor Smyth adopt instead of the quantity of *only* 202 inches, and which he declares was taken by Professor Smyth at that figure *without any adequate reason.*

But Professor Smyth states in answer,—

1. Colonel Howard Vyse did most positively not measure casing stones on *two sides* of the Pyramid's base; he himself does not say, or hint, that he did, and the condition of the rubbish mounds at the Pyramid, testifies to the error of any one who, like the *Proceedings*' author, makes the assertion for him.

2. Colonel Howard Vyse did not measure any lengths about the casing stones which he found on the north side of the Pyramid with "mathematical "accuracy;" for, the casing stones themselves, he and Mr Perring measured only to the nearest whole inch; making the greatest base-breadth = 8 feet 3 inches; and stating elsewhere generally, that the outside of the casing stones is distant from the Pyramid courses of rude and now broken masonry, at that particular part, "about 9 feet." At least that is all that Professor

Libel Third.

In his next paragraph on p. 256, the *Proceedings'* author takes up all the known socket measures of the base-sides of the Pyramid; and as they are precisely those which I have used for the same purpose, and are even taken from my pages,—it might be hoped that there at least, we might agree.

But no! for in the course of the paragraph that author indulges both in a most damaging (if true) accusation against me for my use of the numbers, and in ridicule against the building and its mechanical qualities, for the strange accusation of,—not having been better measured by modern men.

Now the charge against me lies within the simple rules of arithmetic. Three observers, M. Jomard, Colonel Howard Vyse, and Mahmoud Bey, had each of them measured the north side only, of the Pyramid's base, making it 9163, 9168, and 9162 British inches in length, respectively to each observer.

But Messrs Aiton and Inglis had measured all four sides, and made them respectively to each side, beginning with the north one, 9120, 9114, 9102, and 9102 of the same inches long.

From all these numbers then, what shall we conclude to be the length of a mean side of the Great Pyramid's base?

When only the three first determinations were in existence, the plain process was, to take a mean of them; every computer assuming that the other sides of the base were of the same length. But Messrs Aiton and Inglis' measures indicate, *inter se*, that the northern side is longer than the others. Can we, however, trust their measures to the greatest difference indicated, viz., 18 inches?

Smyth has been able to find in the Colonel's books,—and he requests the Royal Society of Edinburgh to ascertain from the *Proceedings'* author where Colonel Howard Vyse has said anything about 108 inches being a *precise and mathematically exact* measure by him of any part of the casing stones, or backing stones, or both together.

3. Professor Smyth under the accusation of having had "no adequate reason" for employing 101 inches rather than 108, as a thickness to be added on to a present masonry course, as he estimated it, to give the ancient bevelled outside surface,—requests attention to pages 22, 23, 24, 25, 26, and 27, of vol. iii. of his *Life and Work* where he had discussed the matter on the best data known to him; and in a work which was in the hands of the *Proceedings'* author when he composed his accusations.

I think not; both because of the necessary roughness of their measuring without having made preparations 2 and 3 (of p. 340); and because their quantity for the north side differs by more than twice that difference from any other person's measures of the same side. But then again, who will vouch for these other measurers? Some persons maintain that civil-engineers of the present day are so superior to those of the past, that Mr Inglis' measures must be the best in the field; others again rather hold the opposite. How then ought the mean of all the just-mentioned numbers be taken? " Shall I take the mean of all four of Mr Inglis' sides as one side, " and so give him a weight of one only, against each of the three " older observers? Or shall I take each of his sides as a determi- " nation as good as each of theirs, and in that case give him a " weight of four, to their one each?" That, however, would not be paying sufficient respect to the exceeding care with which M. Jomard, and the Academicians accompanying him, say that they measured the north side; while again, to give Messrs Aiton and Inglis the weight of *one* only, would not sufficiently mark a sense of approval of their having been the first known men to measure all four sides of the Great Pyramid's base, from socket to socket; and I was bound in honour, and as a witness, to testify for them there. So I settled to give them the weight of two.

I do not pretend to say that I was quite right in that estimation of the probable worth of their measures. But I do mean to say, that having first of all printed those measures pure, simple, and complete—and then stated the principle on which I should take the mean—I took that mean honestly and fairly; and its final result, to the nearest whole inch, came out truly 9142 inches.

But the *Proceedings*' author implies that I did *not* take that mean honestly and fairly; and there is the difference between us. His words are, " he (Professor Smyth) takes two of them (Mr Inglis' " measures) 9114 and 9102—(*but strangely not the largest*, 9120)—as " data; and strikes a new number out of these two, and out of the " three previous measures of Jomard, Vyse, and Mahmoud Bey."

Now, if I had wilfully left out the largest number of the Inglis series, where each member had been considered of equal weight,—I should have been dishonest, and unworthy of fulfilling any scientific appointment. But I did not do so, either wilfully or accidentally. For this is how I proceeded to take equal account of each and every one of the Aiton and Inglis' sides, while I gave their whole, the predetermined weight of two, against the one of each of the other three observers.

Aiton and Inglis,	9120	Jomard, . .	= 9163·
,,	9114	Vyse, . .	= 9168·
,,	9102	Mahmoud Bey, .	= 9162·
,,	9102	Aiton and Inglis, mean of	= 9109·5
		Same repeated, .	= 9109·5
Mean to tenths,	=9109·5.	Mean, . .	= 9142·4
		or to nearest whole inch,	= 9142· *

Now here is no *strange* exclusion of 9120, when 9114 and 9102 are admitted; and, in fact, such an unwarrantable piece of cooking as that would have been, never entered my head, until I read it charged upon me by the *Proceedings*' author. It is, in fact, a pure invention of his, so far as I know or can see—though I must request the Council of the Society, who have printed the erroneous accusation to ascertain what its author has to say in his defence.

Reported additional Authority.

After venturing the statement at the end of the same paragraph, on p. 256—that the above mean length of 9142 inches, is not derived by " direct measurement, but by indirect logic"—the *Proceedings*' author adds to my " Life and Work " data, the following particulars from a subsequently printed number of the *Athenæum*:—

" Lately, Sir Henry James has shown that the length of one of " the sides of the pyramid's base, with the casing stone added, as " measured by Colonel H. Vyse,—viz. 9168 inches—is precisely " 360 derahs, or land-cubits of Egypt; the derah being an ancient " land measure still in use, of the length of nearly 25½ British " inches, or, more correctly, of 25·488 inches."

In this sentence, many persons, recognising the name of the principal head of the Ordnance Survey, may think that there is a new and very authoritative determination of the length of the base side of the Great Pyramid. But a little examination will soon show,—*first*, a slight inaccuracy; for 360 times 25·488, or 9175·68, is not the *precise* equivalent of 9168 inches; *second*, that it (9175·68) does not lie within the limits of *any of the known socket measures* of the Great Pyramid; and *third*, that the derah of 25·488 British inches long, is not known to be an *ancient* Egyptian land measure.

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* While using this quantity in most calculations, I usually state that it is uncertain, from the large difference of its factors, to the extent of ± 25 inches. With such differences, we need not descend to fractions; except where, as above, the *doubling* decided on brings the fraction up to a whole inch.

For on what does the alleged statement that it is such a measure depend? So far as I can ascertain, merely this: that Sir Henry James saw the length and name mentioned—in one of Weales' rudimentary treatises giving the weights and measures of all nations for 1*s* and 6*d*—as a *modern* Egyptian measure. But more important works on modern Egypt, as Lane's and Wilkinson's, do not acknowledge, so far as I can find, such a modern land measure, neither do they mention it as belonging to past history. While all of the works together, or Weales' also as well as these last, agree that *the* standard national cubit of *ancient* Egypt was very near to 20·7 British inches in length, or totally different from 25·488 of the same inches.

Hence I am inclined to fall back after all on my former conclusions from the Pyramid socket observers above stated; or to believe that the true length of a mean side of the Great Pyramid, when properly measured under guidance of the 1, 2, and 3 of p. 340, will be found to lie within the limits of 9142 ± 25 British inches: *i.e.* the present uncertainty is within $\frac{1}{365}$ of the whole. A large proportional error—compared to what it might be reduced to any day, if the Government were consenting to spend on the measures the merest fraction of what the surveying of this country costs them every year; but only a small proportional error—as contrasted with what affected our knowledge of the Great Pyramid 100 years ago; and sufficiently limited to enable us to test, with considerable approach to sufficiency, several theories already before the world.

SIZE OF THE EARTH.

In the course of my investigations into the nature and objects of the Great Pyramid, I have had—beginning with 1864—to compare its measured size in parts, with definite portions of the length of the earth's axis of rotation. The length of that axis I have taken, successively, from means of the best determinations thereof by the greatest geodesists of the age, giving their names, and generally their original quantities; and my adopted mean has always been within the limits enclosed by their several measures; these, slightly differing from each other in the smaller numbers—but so slightly as not to prevent the earth's polar axis being now considered an exceedingly well determined quantity. That is, it is known within a very small fraction of the whole length; which is all that man with his merely finite powers can expect to attain to in any practical problem, whether it refers to the length of a moderate bar of metal, or the dimensions of a cosmical globe.

Accusation First.

Nevertheless, the *Proceedings'* author accuses me of having acted so flagrantly as to have used in my researches "a *supposititious* polar axis of the earth," p. 261; also a something which Captain Clarke (an admitted authority) "did not find it to be;" and finally he states, on his p. 263, that the polar axis of the earth "is still itself an unknown and undetermined linear quantity."

This is the three-fold charge, and now I have to show that each item is utterly erroneous, and is followed by worse things still.

1. As to having used a "supposititious," instead of the measured, length of the earth's polar axis,—I point to p. 450, vol. ii. of my "Life and Work,"—where I have quoted at length, and have followed in vol. iii. the results of calculations based on all the best known measures of the earth, from the worthily authoritative quarto recently published by the Ordnance Survey Office under Sir H. James,—chiefly computed by his excellent mathematical assistant Captain Clarke.

Accusation Second.

2. But it is next said by the *Proceedings'* author, that I, having employed 500,500,000 British inches for the length of the polar axis,—have used for it, that "which Captain Clarke did not find it "to be." Let us see if this charge is true, from the pages of the *Proceedings'* author himself. He states that Captain Clarke found the length of the earth's polar axis by one mode of computation

= 500,522,804; and by another mode 500,482,296 British inches. The *Proceedings'* author does indeed considerably confuse his explanation of the principles of each method; and wherefore the final difference out of the same observational quantities to begin with, —but his numbers are correct, and are probably derived from my own pages with their peculiar mode of expressing Captain Clarke's results of feet or yards, in inches, and showing them, sometimes for the whole axis (vol. iii. p. 134) and sometimes for the semi-axis (as in vol. ii. p. 450.)

Now, Captain Clarke himself does not attempt (as I had already shown in "Life and Work," vol. ii. p. 451) to decide entirely between the two calculated results he has given,—but he states each of them, and seems to leave his readers to do as they like between the two. Anywhere between the limits of his two quantities, therefore, is to be looked on as derived from him, and as, in fact, his determination, rather than any one else's, especially if with some reference to his limits of error or variation.

Now the exact arithmetical mean of the Captain's two published quantities, stands thus—

First computation,	500,482,296
Second ,,	500,522,804
Mean,	500,502,550

or differing from 500,500,000, by only $\frac{1}{16}$th of the amount of the debateable ground between the two almost equally important factors given by him. If, moreover, a very little increase of weight be extended to the first result, on account of the rather higher analysis by which it is brought out by Captain Clarke, the tendency is to reduce even that small difference, or perhaps throw it to the opposite side. Wherefore, if any one will only trouble himself with as many rough digits as he can be certain of,—Captain Clarke's whole result may be stated in a compendious manner as 500,500,000 with a probable error of ± 20,000 British inches.

If then I had stated the length of the earth's axis as outside all Captain Clarke's debateable ground between his two results, say at 500,550,000 or 500,450,000 inches,—the *Proceedings'* author might have truly said that I had stated it at a figure which Captain

Clarke did not find it to be; but when I stated it close to the middle of his debateable ground, and after giving his two limits of that ground,—this second charge of the *Proceedings'* author is no truer than the first; and appears to me a deliberate misrepresentation of the numerical results before him.

Second Part of Accusation Second.

2. 2*d part.*—Not content, however, with making the charges indicated above, the *Proceedings'* author, on his p. 261, asks—

" But is there any valid reason whatever for fixing and determining as an ascertained mathematical fact, the polar axis of the earth to be this very precise and exact measure (viz., 500,500,000 British inches), with its formidable tail of nothings?"

And then he answers, with implied crimination, "None, except the supposed requirements or necessities of Professor Smyth's pyramid metrological theory."

But upon which Professor Smyth remarks, that—*first*, he has not done what the author describes in his question; but, as already shown, has taken his numbers from the results of computed measures of practical observers; and *second*, it is an uncalled for calumny to say that Professor Smyth's pyramid metrological theory requires, or necessitates, that there should be any particular or very precise number, and with many 0's at the end of the British inches in the length of the earth's axis of rotation. From his first quotations from Airy, Schubert, Herschel, Bessel, and others, down to his last from the Ordnance Survey volume, Professor Smyth, in prosecution of the Pyramid metrological theory, has merely sought to know how many British inches long, those who have measured the said axis, by the nearest approximations that can be made,—have found it to be; and whatever the latest and best measurers have found and proved it, he has always accepted; and he is still ready to act in the same manner, for any geodesical results of the future, if they shall be proved intrinsically better than the existing ones.

Accusation Third.

3. Further on, however (at p. 263), comes a more wide-sweeping libel; and now against, not Professor Smyth, but Captain Clarke, Sir H. James, and all the greatest geodesists of every country

throughout the civilized world; for after all their efforts during the last fifty years, and notwithstanding the stupendously large surveys they have been carrying on, at an enormous expense to the nations concerned, in order to ascertain the size and shape of the earth,—the *Proceedings*' author declares, that the length of the polar axis thereof, "is still an unknown and undetermined linear "quantity."

Yet, if we take even twice Captain Clarke's two limits, already given,—the difference of their mean from either one or other, amounts to less than $\frac{1}{10000}$th part of the whole axial length:—a proportional quantity, which, on a foot-rule, would be equal to a lengthening or a shortening of it by much less than the breadth of a hair of the finer human order. And inasmuch as rules are sold in good opticians' shops as foot rules, or as being of the known length of a foot, though they may differ from each other in length by more than ten whole hairbreadths,—why, it is plain that the length of the axis of the earth is to be considered, even as compared with some modern measuring rules, to be remarkably well determined and known. The statement of the *Proceedings*' author is therefore an unfounded slur on the work and reputation of the best astronomers, geodesists, and surveyors of many countries, both in the present and past generations; men who have deserved exceedingly well, both of the scientific and general public, throughout the whole civilized world.

Accusation Fourth.

4. That in the same *Proceedings*' paper, where the length of the earth's axis has been declared by its author to be "an unknown "and undetermined linear quantity,"—there should be the following sentence:—"The engineers and mathematicians of different "countries have repeatedly measured a meridian arc of a degree of "sixty miles, in order to use it as a standard for linear divisions. "As part of their standard, they measure off sixty miles of the irregular earth-surface of a kingdom with almost perfect mathe- "matical exactitude,"—may seem extraordinary.

Extraordinary in the *first* place—because, though a project has been started from time to time, of using some special round and even subdivision of the length of a sexagesimal degree of the meridian for a standard of measure,—it has certainly not become a common practice among nations, nay I doubt whether a single people has adopted it: and extraordinary in the *second* place, because, if such degrees of the meridian can be measured with "almost perfect mathematical exactitude,"—the length of the earth's axis can be computed from them to the same proportional amount of most remarkable perfection; instead of remaining as the *Proceedings*' author says it does remain, "an unknown and undetermined linear quantity."

The question arises, therefore, why has the calculation not been made? And the answer must be—that no one but the *Proceedings*' author knows of any such marvellous data for the purpose: and he is either so inexpert at the computation that he cannot, or so careless of the state of geodesical knowledge in his time, that he does not choose to, make the very great improvement, which his statement implies that he might do, in our physical knowledge, or in our idea of the exact length of the earth's polar axis,—the most important base which men can refer to for measuring the sizes and distances of the sun, planets, and stars.

No one else, I repeat, knows of these almost absolutely perfect data. Such earth-axial knowledge as that represented by the last volume of the Ordnance Survey, is based certainly upon the measured lengths of arcs of the meridian,—but not depending on measurements of one degree only. On the contrary, there is a class of errors concerned, whose proportional effects increase so rapidly, with the smallness of the arc, that the tendency of the age has long been to measure an arc of as many degrees as possible, instead of only one degree. Hence, both in India, and Russia, Britain, France, and Germany, the arcs measured have been made to contain even 10°, 30°, and 60°, by extending them, where possible, across neighbouring countries. Thus the Indian arc, begun near Cape Comorin, is being pushed northward over the Himalayas. The Russian meridian arc, commenced at North Cape, has crossed the Danube, and is approaching the Mediterranean; while all the states of Europe are at this moment engaged in a grand arc of parallel, stretching from Valentia in Ireland to the Ural Mountains in Russia.

Yet though both Captain Clarke's and other geodesists' results are based on all these large arcs, and which *must* give more accurate results than if the same parties had measured single degrees only,—the *Proceedings*' author pronounces their final deductions worthless, in so far as, in spite of them and the faultless formulæ of computation employed, the length of the earth's axis is "still an "unknown and undetermined linear quantity;"—what, then, must not be the overwhelming excellence of the single degree arc-of-the-meridian measures, which the same fault-finding author describes in the same critical paper, as endued "with almost perfect mathe-"matical exactitude?" Why, of course, they must be supernaturally perfect, *i.e.*, if they exist. But do they exist? At present we have only the word of the *Proceedings*' author that they do. And why has he favoured the world, through the Royal Society of Edinburgh, with his statement to that effect?

"Because he knows of their existence, and can produce them," ought to be the answer. But if that answer be not rendered? Why then we have only to look to the sentence following that one which we quoted near the head of this article 4; and there stands a reason of a very exceptionable order, and which manifests its nature increasingly in the latter portions of the *Proceedings*' paper. That reason is given in the words,—"Professor Smyth holds that the "basis side of the Pyramid has been laid down by *Divine* authority "as such a guiding measure."

Accusation Fifth.

In the above sentence then, we have touched at last on the origin of the unnatural excellence attributed to certain unknown surveyors' and mathematicians' measures of 60 miles long: viz., Professor Smyth holds that something about the designing of the Great Pyramid was due to *Divine inspiration* afforded to its architect;* and immediately the *Proceedings*' author insinuates that just such a something is often accomplished by unassisted man in the present day, and carried out by him both on a vastly larger scale, and with infinitely more accuracy.

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* In the same manner as we read in the Bible, it did occasionally please the Almighty to issue instructions for practical works, showing the pattern thereof, pronouncing the sizes they were to be made in all their chief parts, and sometimes even inspiring the workmen with the requisite skill to prepare them. See Exodus xxxi. 1–11; 1 Chronicles xxviii. 11, 12, and 19, 20; Acts vii. 44; and Hebrews viii. 5.

Of course a line 60 miles long, *is* on a larger scale in mere length, than one side of the base of the Great Pyramid, only $\frac{1}{7}$th of one mile long. But then what is there similar, between measuring 60 miles' length of open country in the latter days of the world, and the building of a transcendent monument in primeval times, with an excellence of masonry never since surpassed, and able to endure and bring down through more than 4000 years proof of a pure mathematical figure of important meaning, besides many physical symbolizations expressed in the size of the whole and its parts, as well as in their astronomical emplacement?

The two things are evidently of a totally different kind, and we should have nothing more to reply to under the head of accusation (5), unless the *Proceedings'* author had further propounded the following depreciation of both the modern measures of the Great Pyramid's base-side, and the said base-side itself; in the following words (p. 256): —

> " Surely it (the base-side of the Great Pyramid) is a very strange
> " standard of linear measure that can only be thus elicited and de-
> " veloped—not by direct measurement, but by indirect logic, and
> " regarding the exact and precise length of which there is, as yet,
> " no kind of reliable and accurate certainty."

Now, the recorded and received measures of the Pyramid's base-sides are, no doubt, as we have already seen at p. 351, sadly rough and rude; but then they are in so far true measures, made by actual surveyors, French, British, and Egpytian, at various periods during the last 70 years, and not results ascertained by "logic."

There is also, at present, as already shown at p. 352, *some* amount of reliable certainty as to the measured length; and if any one desires to have that amount of certainty increased,—let them, especially if they are rich men, adopt the necessary practical means for obtaining it: as indicated on p. 340.

The *Proceedings'* author, indeed, intimates that the length of the Pyramid's base-side *cannot* be obtained by the ordinary human means, viz., direct measurement, but only by indirect logic. How logic, whether direct or indirect, is to procure it, I do not presume to know. And equally am I ignorant why direct measurement should not be equal to the problem; for there, defined by the incised corner sockets, is a length marked in a levelled surface of foundational rock, and capable, when certain modern, adventitious mounds of rubbish are cleared away, of being measured as accurately as any base-line that modern surveyors have measured on any part of the earth's surface.

Therefore, if modern men do not yet know that Pyramid base-side length accurately, whose is the fault, but their own; and instead of vilifying the character of the Great Pyramid, it would be more to the purpose, were they, even now at the eleventh hour, to take shame to themselves, and send out a strong party of surveyors and measurers, to undertake the work they have not yet performed, and with all those same means and appliances, which they employ in any other case where good lineal measures over long distances are required.

THE SACRED CUBIT OF THE HEBREWS.

In his pages 257 and 258, the *Proceedings'* author states, rather ambiguously, that Professor Smyth believes that the length of the base-side of the Great Pyramid, when measured with a standard whose length is the ten-millionth part of the semi-axis of the earth,* exhibits the number of days and parts of a day in the year; that that standard is the Pyramid Cubit; that it is the same in length as the Sacred Cubit of the Hebrews—such cubit measuring 25 Pyramid inches, = 25·025 British inches; and that the whole idea, apparently, of the metrical theory of the Great Pyramid, is built upon the correctness of this Hebrew cubit datum.

The author then goes on more distinctly to attack that datum, and to show that it is exceedingly incorrect: That Sir Isaac Newton came long ago to the conclusion that the true length of the Sacred Hebrew Cubit was 24·82 inches, from which it must not be altered: And that Professor Smyth, in taking the means of Sir Isaac Newton's original quantities, and representing them as 25·07

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* According to Captain Clarke's two limits already given, such fraction must lie between 25·026 and 25·024 British inches; wherefore I have usually taken 25·025 ± ·001 British inches as the practical quantity to apply: in so far I believe in accordance with the eminent and exemplary authority of Sir John Herschel.

± ·10 British inches, made a gross mistake; for the mean, of certain intermediate numbers printed by him, is really 25·29.

Now, in the first place, it must be seen pretty clearly by most persons that the length of the Sacred Cubit of the Hebrews cannot be really necessary, in a direct way, to the metrical theory of a building erected long before the time of Abraham. And, indeed, the modern Pyramid scientific theory was elaborated without any dependence upon it, and still stands on its own foundation and references to science. But after the Pyramid theory was so far worked out, and had produced a Pyramid cubit of a particular length,—then that length was compared with other cubits: and, though found utterly different from the cubits of Egypt, Babylon, Assyria, and other pagan Gentile nations,—yet came very close to what Sir Isaac Newton had deduced for the length of the *Sacred* Cubit of the Hebrews.

Sir Isaac Newton's treatise was therefore examined, and while it was found that he looked upon his adopted result as only temporary, imperfect, and even distinctly to be replaced at a future time by some other person's completer researches,—a careful re-examination of his data, according to modern methods, led Professor Smyth to a slightly different quantity, as being a better approximation to the truth therein enveloped, viz., the length of the sacred Cubit of the Hebrews, than that quantity which Sir Isaac had employed. This difference was not of an amount to alter our most eminent philosopher's grand conclusion as to the total difference between that Sacred Cubit of the Hebrews, and the profane cubit of the Egyptians,—but only to affect a residual question of a much minor lineal difference, and which had not occurred to him, or to any one in his day.

I fully believe, therefore, that Sir Isaac Newton himself, were he still alive, would allow that my deduction of 25·07 ± ·10 British inches, from his several data, expressed chiefly in Roman unciæ, is a truer result than his of 25·6 Roman unciæ, interpreted variously by modern authors as anywhere between 24·75, 24·82, and 24·89 British inches.

In order that there might be no mistake about what Sir Isaac Newton's opinions and data were, I reprinted in the 2nd vol. of "Life and Work," the whole of his original paper: and only commenced the discussion upon them in a subsequent part of the volume. In that portion of my work, it has recently been discovered that there is an unfortunate misprint of certain intervening computed quantities, between Sir Isaac's original data, and my final deduction from them; some erroneous slip of paper having apparently got introduced into the MS. finally sent to the printer. I acknowledge that there is such an error, thank those cordially who have discovered it; and have already rescinded it from the volume by printing it as an error in a list of errata, of which list I place a bundle of examples on the table, so that any member who has a copy of "Life and Work," may supply himself with this addition.

A very little attention in collating and comparing, would have indicated to the *Proceedings'* author that these intermediate figures, as they are printed in the 2d vol. of my "Life and Work," p. 458, are simply misprinted: for, they are neither correctly derivable from the original quantities, nor is the following *mean* derived from them. But he has chosen rather to suppress the fact of my having given, in the same volume, Sir Isaac Newton's original numbers, and given them correctly and completely, to the extent of several pages; omits Sir Isaac's statement that he looked on his one chosen and adopted quantity as temporary,—parades in *Proceedings*, p. 258, my misprinted intermediate figures, as if they were all important original ones; and glories in pointing out that the *mean* put by me underneath them, is not the correct mean of those numbers. Of course it is not; because it was derived from other numbers. And as both that final mean of mine is rightly printed, as intended, and the original quantities are also correctly printed,—what occasion was there for bringing up this accidental misprint of some intervening figures so very prominently, and alluding to it as something radically subversive of the whole theory of the Great Pyramid?

To assist those who may try to ascertain, I subjoin, in a note, a description of the numerical particulars of the case, prepared originally for the *errata* list in "Life and Work;"* and now I pass on to the consideration of the next principal section.

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* ***Sir Isaac Newton's Numbers for the Length of the Sacred Cubit of the Hebrews.***

At p. 458, vol. ii. of ***Life and Work at the Great Pyramid***, there is an unfortunate misprinting of the calculated numbers representing in British inches,

SIGNIFICANCE OF CYPHERS AND FIVES.

Under the above head, p. 264, the *Proceedings'* author has some remarks as to certain persons, in their comparisons of lengths in the Great Pyramid, with the size of the earth, imagining " that if

the quantities from which the mean "25·07, ± ·10 British inches," for a new statement of the length of the above cubit, was derived. This *final* mean is correctly given, as intended; so likewise are the *original* terms, expressed chiefly in Roman Unciæ, in Sir Isaac Newton's Dissertation on Cubits, reprinted at pp. 354–366. No important mischief therefore is likely to have accrued, from this error in printing one of the *intermediate* steps. But as the error is an undoubted blemish, which I much regret, have cancelled in the list of *errata*, and sincerely thank those who have called my attention to it,—I hasten to give the following discussion *de novo.*

At p. 365, of Sir Isaac's treatise above mentioned, he assumes 25⅗ Roman unciæ, to represent the length of the Sacred Cubit of the Hebrews,—a cubit which he had elsewhere shown, there were grounds for believing that that people possessed before they went down into Egypt, and had had specially brought to their attention again, for religious matters, after leaving Egypt under Moses.

But Sir Isaac Newton was not at all confident of having obtained the precise length, to the last figure put down in his arithmetical expression. And he particularly and almost prophetically says,—

" This is what I thought proper to lay down *at present* with regard to the
" magnitude of this cubit. Hereafter, perhaps those who shall view the
" sacred mount, and the monuments of the *Chaldæans*, by taking accurately
" the various dimensions of the stones, bricks, foundations, and walls,
' and comparing them together, will discover something *more certain and*
" *exact.*"

Now what Sir Isaac laid down at that then present time, was abundantly sufficient for his then purpose; or to prove, that there existed a most sensible and positive difference in the length of that sacred (or 25⅗ unciæ) cubit of the Hebrews,—and, of the profane cubit of the Egyptians, whose length, expressed in the same Roman unciæ, was hardly more than 21·3. And in this last conclusion, he is so eminently borne out by all subsequent investigators, that that subject—or the length of the *profane*, or ancient Egyptian national cubit—need not be stirred again.

But within the last few years, another, and a more refined, or a residual question has arisen, which apparently never crossed Sir Isaac Newton's mind, viz., was the Sacred Cubit of the Hebrews, taken by itself, accurately the ten-millionth part of the length of the Polar semi-axis of the Earth? And as this quantity in Nature, according to modern science, is something very close to 25·8 Roman unciæ,—Sir Isaac's determination of 25 and ⅗, *i.e.*, 25·6 of the same unciæ for the Sacred Cubit, is, to say the least of it, so near—especially for a confessed imperfect approximation, from a portion only of the materials collected,—that it becomes intensely important to submit *all* the data to a more rigid scrutiny than before; with the caution moreover in view,

" by multiplying one of their measures or objects, they can run the " calculation into a long tail of terminal 0's, then something very " exact and marvellous is proved." This he derides, because, ac-

of assigning some limits, within which we may feel tolerably certain of the result.

The several quantities therefore, extracted from my reprint of Sir Isaac Newton's paper (but to which in the original, I cordially refer all readers), and reduced to British inches—at the approximately assumed rate of 12·15 for 1 Attic foot; and 0·97 for 1 Roman *uncia*—are as follows:—

No. of Approximation.	METHOD OF REFERENCE.	Attic Feet.	Roman Feet.	Roman Unciæ.	British Inches.	British Inches.
						Mean of limits.
1.	From Talmudists and Josephus, by the Greek cubit (p. 355), Vol. ii., of *Life and Work*, between	2⁴⁄₇ and 2	... ...	... ...	31·24 24·30	27·77
2.	From Talmudists, by proportions of human body (p. 355), between		... ..	28⅘ 24·	27·94 23·28	25·61
3.	Josephus by pillars of Temple, in proportions to the human body (p. 356), between		2⅓ 2	28·0 24·0	27·16 23·28	25·22
4.	By Talmudists' and "all Jews'" ideas of a Sabbath-day's journey (p. 358), between . . .		2⅓ 2	28·0 24·0	27·16 23·28	25·22
5.	By Talmudists and Josephus, on proportion of steps ascending to Inner Court (p. 358), between			27·0 24·0	26·19 23·28	24·74
6.	By many Chaldaic and Hebrew proportions to Cubit of Memphis (p. 362) .			25·6	24·83	24·83
7.	From a statement by Mersennus, as to the length of a supposed copy of the Sacred Cubit of the Hebrews, preserved in a secret way from the knowledge of the Christians (p. 363) . .			25·68	24·91	24·91

The simple mean of the last column, = 25·47 British inches. But that is not a proper method there; because, not only has Sir Isaac Newton evidently cording to him, "many small objects, when thus multiplied sufficiently, give equally startling strings of 0's;" and he then goes on to furnish three such examples; the first being made gratuitously

shown that he had most confidence in his two last determinations; but his first, by its very wide limits, shows that it is by far the least trustworthy of all. Some decrease of weight, therefore, for No 1, and increase for Nos. 6 and 7, require to be made. How much precisely, it is impossible to say: but perhaps ⅓ for the former, and 3 for each of the two latter, the intermediate quantities being reckoned at 1 each,—may be considered fair and probable. In which case the mean comes out, 25·05 British inches.

While, simply,—and in fact as I did on the first occasion, using then a slightly different value of the Roman uncia,—throwing away the one very objectionable observation, and taking a mean of the rest, unweighted, gives 25·09 of the same inches.

But neither 25·09, nor 25·05 are fully safe, either in the second, or perhaps the first, place of decimals;—for—besides the uncertainty connected with the proper weighting of each of the results, according to the different kind of documentary evidence obtained by Sir Isaac Newton on each occasion,—there is considerable uncertainty in the value of a Roman uncia, expressed in British inches. We have assumed as above, that the former = 0·97 of the latter: but modern scientific and architectural authorities are found anywhere, between Zach at 0·9681 and Penrose at 0·97286; and might require us to reduce our final quantities by — ·05, or increase them by + ·06 of an inch; or by any intermediate figure.

Wherefore, the statement already printed at p. 458 of vol. ii. of *Life and Work at the Great Pyramid*,—*i.e.* 25·07 ± ·10 British inches, for the best result deducible from all Sir Isaac Newton's approved approximations for the length of the Sacred Cubit of the Hebrews,—is, if not as good a statement as can be made,—at least a great deal better than the 24·82 inches, absolute, which has been hitherto current in most English works; and beyond comparison better than the 20·7 inches, nearly, of the ORDNANCE SURVEY *Map of Jerusalem.*

This Ordnance quantity of 20·7 inches is evidently not the *sacred* cubit at all, but the *profane* cubit; and in the explanations of the scale at the foot of the above map, the revered names of "Sacred," and "Cubit of the Tabernacle," are given to precisely what Moses was so anxious to keep them from being confounded with—viz., the cubits of idolatrous Egypt and other Gentile nations; the inscriptions at one end of one of the Ordnance-map scale-lines being—"*Egyptian, Hebrew, Babylonian,*" and at the other end of one and the same line—"*Royal or Sacred Cubits*, also *named Cubits of the Tabernacle.*"

If this map is one of those prepared, as believed by some, at the expense and to the orders of the Fathers of the Palestine Exploration Association—such a radical error with regard to the *sacred cubit of the Hebrews* may well excite surprise. But if, on the contrary, the map is purely the work of the several Ordnance officers whose names are conspicuously engraved upon it—the nation must regret that they should have so entirely ignored the researches of Sir Isaac Newton, the greatest philosopher their country ever produced, and in one of the most important of all questions that have ever been brought forward in either the science or history of metrical standards.

to insult religious feelings and scientific ideas, in its mode of reference to Sir Isaac Newton's *Sacred Cubit* of the Hebrews,—the cubit according to whose standard Sir Isaac believed that the true God was once pleased to define to men the size of the Ark of the Covenant, the Tabernacle, and the Temple of Solomon, in the several revelations averred in the Holy Scriptures to have been made for that end both to Moses and David. As insulting, I am sorry to say, to other men's, both religious and scientific, ideas as possible; for what is it, which the *Proceedings'* author brings up as being in length a half, or as good as a half, of that *sacred* primeval measure of the earth,—why his own ill-defined and flexible hat. And at the time of delivering the discourse, of which the *Proceedings'* paper purports to be only a part representation of the substance, its author even went through the hypocritical form of demurely measuring the said variable and inexact hat (placed, together with a measuring scale, on his reading desk by an assistant, before the lecture began), and then finding, with smiles all over his countenance, before the members assembled, that the larger diameter of said hat's brim was just half of the "Sacred Cubit!"

This act,—much to be regretted,—has crowned itself with confusion in the *Proceedings'* paper, and at the performer's own hands, by all his three examples failing miserably.

Thus the misguided, though in other things learned doctor, says —that his hat's brim of 12·4 inches in length, is 1-20,000,000th of the earth's polar axis, that being taken at 500,000,000 inches; but any school-boy may see that it is rather the 1–40,322,581st part thereof; a result where there are few 0's, and a very large difference at the beginning.

Next, he says that "a page of the print of the Society's *Trans-*" *actions* is 1–60,000,000th of the same." Here, indeed, he prudently avoids giving the measure of the said page; but I, having measured it on the same principles as his third case indicates, find the limits as being not less than 8·37, nor more than 8·43 inches, on complete pages of a recent volume. Wherefore taking the mean = 8·4 inches,—that will be found, not the 1–60,000,000th even, but the 1–59,523,810th part of the same axial length.

And finally he states that "a print page of Professor Smyth's "book, 6·2 inches in length, is 1–80,000,000th of this *great* "*standard*; &c., &c." But 6·2 inches form, evidently, not that even fraction, but the 1–80,645,161st part of the same 500,000,000. A length, however, this last, in *British* inches (and no others are there alluded to by the British *Proceedings*' author), which neither Professor Smyth stated, nor Captain Clarke, nor any other known geodesist, ever found, the earth's axis to measure. Nor is the length 24·82 inches, as shown in the previous section, the length which Professor Smyth has deduced, or considers to be deducible from all Sir Isaac Newton's numbers, taken together according to modern scientific principles, for the length of the Sacred Cubit.

All the three examples thus given by the *Proceedings*' author have totally failed in producing his promised "startling strings of "0's:" and though he does put "&c., &c." after his last case,—yet if these symbols refer to other examples of the same order as his previous ones, they will not strengthen his case.

With a very little cleverness in looking out through the world, no doubt other persons might find some trifle coming more nearly to an even fraction of the earth's polar axis than any of the *Proceedings*' author's given examples. But then it must be remembered, that when the length of that said axis has already been ascertained by national efforts,—there is no more difficulty in finding a practical equivalent to a small fractional part thereof, than in performing a sum in simple arithmetic: and after this little thing should have been done, it would really possess no similarity with the alleged case of standards of measure in the Great Pyramid,—deposited or built in there, on a gigantic scale, 3000 years before any accurate determination of the length of the earth's polar axis had been made by men,—and yet found now to be an apparently exact and even decimal and quinary fraction thereof.

This really astonishing geodesical character of the Pyramid standards, is a matter to be disproved, only by still more accurate measures of both the Great Pyramid and the earth being instituted; and then, perhaps, found *not* to be commensurable by quantities larger than the limits of error of the measures. At present there *is* a coincidence *within* the limits of errors of the best modern measurements of both; but in the case of the Great Pyramid these are not by any means so refined as they might be. The proper course, therefore, for both religious minded, and scientifically inclined, persons is, or should be, not to take to "scoffing and deriding," but to visit the Great Pyramid in a calm and working philosophical spirit, and to remeasure more exactly all those still existing fiducial markings which have marvellously descended to our latter times from primeval ages, closed long before history began.

With regard, next, to the *Proceedings*' author's termination of his section; or, that many arrangements in fives are to be seen in the rooms of the Royal Society of Edinburgh,—*i.e.* in a modern building of a most confused multiplicity of parts, and made up in design by copyings from several foreign, and some ancient, architectures,—and his conclusion that the arrangements were not carried out there for any special purpose of symbolizing five; and "that there is not the slightest ground for any belief that the "apparent *fiveness* of anything in the Great Pyramid had a different "origin;"—I can only spare time to remark that the two cases are almost awfully different; for—

1*st*. The Great Pyramid was built 4000 years ago, in the symbolic ages of the world, according to many indications in Biblical history, and we have no other guide for those very early times.

2*d*. The Great Pyramid's whole mass and whole surface,—and those so huge as rightly to have procured it the title from all the nations of the world of "the Great Pyramid,"—are arranged so as to make two most colossal representations of fives; and have been beheld by nearly all peoples from the beginning of history; and—

3*d*. Compared to the above, either in size, entireness, or publicity —the few fives here and there inside the very modern Royal Institution building of Edinburgh, and in one part merely of which the Royal Society of Edinburgh meets and the fives are said to be chiefly found,—are only like the fives which any child may discover in a modern multiplication table; *i.e.*, they are small both in themselves and in proportion to the other numbers represented, and not dominant over the whole structure.

Arithmetical Means.

When the *Proceedings*' author attacks some of my stated results of Pyramid measures for the reason that they have been obtained by "the equivocal process of means" (p. 259), and insinuates against one particular final quantity—which is the mean of the largely varying results of four different observers,—that it is "an "exact measure which certainly none of its many measurers ever "yet found it (the thing itself measured) to be" (p. 256),—I can only hold up my hands in astonishment.

For here is a person criminating utterly the simple arithmetic process of taking arithmetical means in physical science; and bringing up the special accusation, equally applicable, if at all, in pure, as well as applied, arithmetic,—that the mean does not come out exactly the same as one of its component numbers.

This is surely something worse than advocating perpetual motion, spirit-rapping, or any other absurdity which would not be permitted to enter the books of the Royal Society of London: and indicates that the *Proceedings*' author is running a-muk, in the manner of certain of the Javanese, against the method of averaging employed in every scientific, commercial, and insurance establishment in every country throughout the civilized world. And if the Royal Society of Edinburgh has permitted such sentiments to be uttered at their meeting and printed in their *Proceedings*,—I would beg them to explain, through their Council or otherwise,—if they be lieve the sentiments,—how they maintain them; and if they do not believe them,—why they give them honourable publicity, and against the Pyramid subject alone?

(In the meanwhile* that there may be no doubt of the fact as held by the *Proceedings*' author, I subjoin, in a note,† an extract from one of the daily papers of April 21,—expressing the gentleman's sentiments as delivered to the Royal Society of Edinburgh, on April 20, more fully; and I hope completely relieving me of any necessity for answering him on the point.)

MINUTENESS OF MODERN PRACTICAL STANDARDS OR GAUGES.

Under the above heading the *Proceedings*' author has tabled some objections to results deduced from the Great Pyramid,—but the objections are founded on such erroneous principles of practical science, that I notice them only for the sake of the Royal Society of Edinburgh, in whose publication they now appear.

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* This paragraph in parenthesis, was added after the reading of the paper.

† "It was an erroneous method of procedure to take the mean of different "measurements. Such a method of procedure Sir J. Y. Simpson alleged "was childish: it was a species of mathematical aberration, and it ran "through the whole of Professor Smyth's book."—*Scotsman* of April 21.

The first objection is, that the theoretical and actual sizes of the supposed *standards* of the Pyramid "are made to vary in "different books, *which it is impossible for an actual standard* to do." (p. 265.)

Yet it is a notorious fact that the theoretical and recorded sizes of the actual national standards do vary in different books and even sometimes in the same book; and this, whether we take the pages of micrometric measures for the length of our standard yard from the Philosophical Transactions, or the Memoirs of the Royal Astronomical Society, and compare these with the different ideas of the *savants* of various countries, at different times within the last 100 years, as to the true length of a pendulum vibrating seconds in a particular position on the earth, (the British yard's theoretical reference); or whether we enter into the conflicting ideas of the French nation, at successive epochs, as to the real length of a ten-millionth of the earth's meridian quadrant, and its proportion to their standard metre, at different temperatures.

Thanks to the exertions of the many great men who have laboured splendidly in the cause both of the theory and practice of such standards,—the differences of the modern standards alluded to, are now reduced within very narrow limits. But differences to some extent there are still, and will be for ever, according to the nature of things, touching all practical standards, their theoretical references, and every subsequent, additional determination of the numerical value of the same,—if pushed to sufficient refinement, and whether by one or many observations.

The more man strives after practical perfection, the more impossible it is found to attain to it. An uneducated countryman thinks he has the time *perfectly*, if he knows it to the nearest minute. But an astronomer, though he obtains the time by observations to a fraction of a second, yet always finds his results attended by some still smaller fractional portion of error. Again, a carpenter compares two specimens of three-foot rules, and if they agree within half of $\frac{1}{16}$th of an inch, he at once pronounces them perfect: but a learned man who measures their respective lengths to $\frac{1}{10000}$ of $\frac{1}{16}$th of an inch, can seldom or never reach perfect decision in his last place of figures; and finds anomalies of expansion, imperfections of the eye, or the touch, plasticity of the hardest metals, and all the powers of nature interfering between him and his proposed goal of perfect accuracy.

Well therefore has one of the best men whom Cambridge has sent forth within this century, summed up these ruling facts of all the higher natural philosophy,—by stating "that no human
" hand or machine ever yet drew a straight line, or erected a per-
" pendicular, or fixed an instrument in perfect adjustment." And in truth it is an attribute higher than man's, to perform any practical mechanical operation with perfect exactness.

To acknowledge this fact and work within it,—tends to lead men on to that old and beautiful confession of Sir Isaac Newton, *i.e.*, that all that even he had been able to do, through a long life spent in scientific observation,—was, to pick up a few pebbles on the shore of the boundless ocean of knowledge. It tends, in fact to keep man in his proper place, in humility of mind, but in earnest contemplation of a high ideal, and in efforts always to rise above his last endeavour,—assured that there is hope for some further improvement, when the distance between himself and ideal perfection is infinite.

But to deny the fact of man's incapacity for perfect performance of problems in practical science, and to refuse its evidences—is either to exhibit an uneducated ignorance of what accuracy is,—or to endeavour to apply to the creature that which is an attribute of the Creator.

Hence, when the *Proceedings'* author (at p. 265) lays it down, that
" Measures, to be true counterparts, must, in mathematics, be
" not simply 'near' or 'very near,' which is all that is generally
" and vaguely claimed for the supposed pyramidal proofs; but
" they must be entirely and *exactly* alike, which the pyramidal
" proofs fail altogether in being. Mathematical measurements of
" lines, sizes, angles, &c., imply exactitude and not mere approxi-
" mation; and without that exactitude they are not mathema-
" tical, ——"
we may well ask, Is the gentleman showing simple ignorance of

the difference between pure mathematics and applied? or, Is he unjustly and knowingly stating against the Pyramid's deduced standards, as an acknowledged metrological fact, that of which he knows the very contrary has been found to prevail by all those men who have devoted themselves specially to the most accurate possible mensurations.

Into this nice question I shall not attempt to penetrate,—but will pass on to a statement at the foot of the same *Proceedings*' page, asserting that "our real *practical* standard measures are infinitely " more refined and many thousand-fold more delicate than any indefinite and equivocal measures alleged to be found in the Pyramid by even those who are most enthusiastic in the pyramidal " metrological theory."

This is clearly stated there, and then its writer brings forward as proofs of that *infinitely* superior accuracy, refinement, and delicacy, the thickness-gauges of Mr Whitworth.

Now these gauges are no doubt very admirable things in their way and in their own place; and as they range generally from 0·2 to 6·0 inches in diameter, they are exceedingly useful for testing the thickness of iron rods and plates in engineers' workshops. But they are not yet employed by the Government as the standard length of the country; they would form also a most awkward shape for a standard to refer to, in measuring any long length on the surface of the earth; and from their shortness, would require to be multiplied so often as to lead to serious chances of error; while finally comes the question, are they adapted to last?

A national or scientific standard ought to hand down the national measure unchanged for thousands of years. But will so oxidizable a substance as iron, and this is what Mr Whitworth employs, be safe through such long ages? When so little of the iron of the Greeks and Romans, and less of the Egyptians, has come down to our time—grave fears may be entertained for the Manchester iron plugs and sockets.

Now the standard of the Great Pyramid, on the contrary, comes before us with the proof on its brow of *having* stood that test, which Mr Whitworth's thickness gauges have not yet gone through; —viz., of having lasted before the world, and in spite of conquering and insulting nations, for 4000 years. In spite too of conquerors who did everything they could to annoy and crush the patriotic spirit of the Egyptians, by ransacking their tombs, polluting their temples, overturning their statues. And certainly, if by leaving out to rust in the dewy night-air a few iron gauges, the Persian Cambyses could have spoiled, nay destroyed, the Egyptian national measure of length,—would he not most assuredly have done so?

This *first* point of comparison then, is no problematical, but a certain and proved, superiority of the Great Pyramid over any little iron gauges for national standards. And a *second* similar point is, that taking account of all the various lengths which man has to measure, and the importance for accuracy of having to multiply the standard as little as possible,— a length like a side of the Great Pyramid base, of more than 9000 inches, and that in a material preserved by nature at a practically equal temperature and constant length,—must be preferable to something that a man might put into his waistcoat pocket; but have proportionally large temperature and other corrections to apply to it.

In the *third* place, it is not right to say that our modern practical standards are many thousandfold more delicate, and *infinitely* more refined, than those of the Great Pyramid. For while some of the modern standards, when they have passed out of the hands of their makers, have been found on testing after a few years to be from $\frac{1}{10000}$th to $\frac{1}{100000}$th wrong, and while the ordinary divided scales in opticians' shops may be found from $\frac{1}{300}$ to $\frac{1}{5000}$th wrong,—the base-side of the Great Pyramid, according to the description of its masonry by Colonel Howard Vyse (see note p. 325), must have admitted of measure to less than 0·01 of an inch at either end, or equivalent to $\frac{1}{500000}$th of the whole; *i.e.* if, in all justice, the same refined means of now making the measurements were to be employed there, as are so freely spent on our ordinary modern standards at home.

Evidently, in fairness, either the Great Pyramid's base-side should be as carefully measured, and with reference to the state in which its builders left it,—as are our modern standards with regard to their makers; or the modern standards should be treated as the Pyramid's base-side has been hitherto, and have a few thousand tons of rubbish accumulated over them; in which case who could even guess what size they might or might not be. In either case the powers of the Great Pyramid's base-side, for accuracy and lasting quality combined, would probably be found vastly above those of any standard of measure at present known to be possessed by any nation in Christendom.

PYRAMID ALLEGED TO BE A SUPERHUMAN, AND MORE OR LESS AN INSPIRED, METROLOGICAL ERECTION.

To the above very serious title the *Proceedings*' author arrives in the last section of his paper (p. 268); and seems to decide that Khaliph Al Mamoon by breaking in, "upset the supposititious "miracle," (of the Great Pyramid, as begun to be interpreted by the late John Taylor), "a thousand years ago;" also, that something else was recently discovered in the building, "some thou-"sand years too late for the evolution of the alleged miraculous "arrangement;" and finally, that Professor Smyth has been "wrongously and disparagingly attributing to Divine Inspiration "and aid, things that are imperfect and human."

When from nearly first to last, in that over remarkable paper in the *Proceedings* (the second of the two papers on which I have been discoursing this evening),* there has now been shown proof that it is characterised by erroneous statements, perverted meanings, unworthy insinuations, and denials of some of the most commonly known principles of arithmetic, and most widely followed methods of physical science,—the members of the Royal Society may see, that there is no possibility of my discussing the Pyramid question generally with that gentleman on *scientific* grounds.

As little can I attempt to do so on a *religious* foundation. Allusion indeed to all religious argument the Society ordinarily eschews, though the *Proceedings*' author was allowed to introduce it into his pages 267 and 268, besides inserting several rather scoffing and derisive passages at pp. 254, 256, 260, and 264. But even though the Society should give me equal liberty to answer,—no discussion of a peaceful, improving kind can be instituted without some first principles being agreed on. And as the rationalistic doctrines, and *a priori* theories laid down by the *Proceedings*' author, are totally opposed to my inductive gatherings of what *are* the characteristics of the only generally admitted example of Divine Inspiration, viz., the Sacred Scriptures,—I decline to argue that part of the question with that gentleman at all.

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* The present essay, especially in its latter half, was much shortened on the occasion of reading before the Society on April 20, in order to bring it within the limits of time allowed. All the leading remarks of it were, however, noted, and are now again brought forth with fuller materials for proof or disproof.

To the Society, therefore, only would I appeal,—after what they have already printed and published,—earnestly inquiring of them why they patronize arguments which, if of force against the supposed primeval, and pre-Biblical, Inspiration of the design of the Great Pyramid, are of equal force against the Bible itself?

How, for instance, can it be justly maintained by those who hold to the Bible, that Al Mamoon's breaking into the Great Pyramid, and exercising a power over it, to injure it,—proves the structure *not* to have been built originally by Divine Inspiration,—when the Bible relates that the Temple of Solomon, though actually so built, or according to plans furnished in number and measure by Divine Inspiration, was nevertheless destroyed by the Babylonians, and its sacred vessels carried away, to be used in the service of idols; and now, together with the Ark of the Covenant, they have vanished from the surface of the earth?

Does any one venture to assert that Al Mamoon "upset the "miracle of the Great Pyramid by, as some say, unveiling, a thousand years before the appointed time, the contents of the King's "Chamber?" Let such persons first prove that Al Mamoon did unveil, or even see, any of *those* contents which are supposed to have been above the powers of unassisted men to have constructed intentionally at the date of the Pyramid's foundation,—viz., the earth and heaven commensurabilities of the Pyramid's several parts. These proportions have only begun to be thus recognized, so far as I know, within the last 10 years; and Al Mamoon no more saw them or published them to the world, when he broke into the Pyramid by brute violence in 830 A.D.,—than did Pompey, when he burst into the Holy of Holies in Jerusalem, see that presence into which he would have forced himself impiously.

Does the critic object,—that modern men find such exceeding difficulty in trying to ascertain the exact lengths and breadths of the various symbolical parts of the Pyramid,—that they settle, and consider themselves entitled to settle thereby, that the structure could not have had an all-wise prompter, planner, or designer;—let the critic confess whether there are not many words and passages both in the Hebrew and Greek Scriptures, on which neither divines nor scholars have yet been able to come to a full understanding, and where they are still bringing forth their various readings!

Or, because the Great Pyramid has not been successfully interpreted by mankind during so long a period as 4000 years; or because, during all that space of time its presumed mission has not been accomplished in the world, and so many centuries have passed away fruitlessly since the seed was sown, in an age before any human intellectual history began,—shall that be considered a good *Biblical* reason against there having been Inspiration at the beginning, and against a Divinely intended result still to appear,—seeing that the Bible has announced many prophecies, which are not yet fulfilled; and tells us repeatedly of mysteries which were prepared for, from the very beginning of the world, and yet only made manifest in latter times?

But I will not attempt to follow any further, or to set in order, the arguments of which there are so many, connected with Revealed religion;—for I am too much grieved and humbled to find, that the most conscientious labours and the justest deductions that I have been working at for years past, and whereby I had hoped to be instrumental in affording to some other earnest thinkers a ray of light on a sacred, as well as a scientific, topic,—have issued before the Society merely in this, that my labours, their subject, and my friend's religious convictions have all been dragged contemptuously in the dirt, and made a mockery of, in the fearful manner accomplished by the *Proceedings'* author.

Of *Revealed* Religion therefore I will not presume to say more in this assembly: and I hope pardon may be extended to me for what I have said, if imperfectly, or mistakenly, yet with the best intentions.

But I would beg, before concluding, to remind the members, one and all, of a remark by the notable Kepler, bearing on *Natural* Theology. The existing state of our Great Pyramid knowledge is very similar to what prevailed with regard to astronomy in the time of that acutest genius of mediæval Germany; for, observations were then rough, and rude hypotheses succeeded each other as found from time to time best adapted to explain all the known facts, and were not yet worked up into a complete theory.

So many failures had at that time already taken place, that when success did come at last, hardly any one would believe Kepler's brilliant discovery of the elliptical motion of the planets. His friends would hardly allow that he was sound in his mind; and yet his discovery proved the morning star of the epoch of the Law of Gravitation. They ridiculed and despised: but thus he comforted himself in a knowledge of the reality, and a presentiment of the importance, of what he had learned; and with an intellectual enthusiasm, not often seen to an equal degree in the present day of a more delicate education, he thus breathes forth his noble spirit:—

"Nothing holds me. I will indulge my sacred fury! If you "forgive me, I rejoice; if you are angry, I can bear it. The die is "cast. The book is written, to be read either now, or by posterity. "I care not which. It may well wait a century for a reader, since "God has waited 6000 years for an observer!"

Not more than 4000 years has the solution of the mystery of the Great Pyramid waited; but yet what an interval is that, as measured by nations and peoples, and languages and tongues. Through all that mighty period, what other building than the Great Pyramid has brought down to us in the present day a symbolical language of forms, and accurate details capable of still giving forth exact, contemporary, and high scientific information of that early time?

Surely the Great Pyramid building is unique in its majesty. Take it all in all, it is solemnly alone!

The second author in the Society's *Proceedings* referred to does, indeed, indicate in his very last sentence, p. 268, that he knows of just as good traces of equally advanced "architecture and astro- "nomy" belonging to the same age, "*in various parts of the* "*East.*" I can only hope that the Council will examine him on that point;—for, according to the best of my researches among the writings of those who have explored the Eastern lands—no such examples have there been found.

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3. On the Products of the Destructive Distillation of Animal Substances. Part V. Action of Sodium on Pyridine. By Professor ANDERSON, Glasgow.

4. On the Structure of the British Nemerteans; and new British Annelids. By W. Carmichael M'Intosh, M.D. F.L.S.

In the first part of this paper the anatomy and physiology of the Ommatopleans is described from the typical form *O. alba*, the variations and peculiarities presented by other genera and species being contrasted therewith. The descriptions are grouped under the following heads:—Dermal Tissues; Proboscidian Sheath; Proboscis; Digestive, Circulatory, and Nervous Systems; Cephalic Furrows, Pits, and Glands; Organs of Reproduction and Development.

The second part consists of the structure and physiology of the Borlasians, under the same (or similar) heads; together with such anomalous genera as differ from both great groups of Nemerteans.

The third division treats chiefly of the structure of certain (upwards of forty) Annelids new to science or to Britain.

The following Gentlemen were elected Fellows of the Society:—

Rev. THOMAS GUTHRIE, D.D.
THOMAS KEY, Esq., Dep.-Inspector Gen. of Hospitals.
ADAM GILLIES SMITH, Esq., C.A.
JOHN MACMILLAN, Esq., M.A.

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